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FROM BIOLOGY TO MATHEMATICS *

J. H. WOODGER

I

IN Volume II, No 7, of this *Journal*, in an article entitled 'Science without Properties,' I showed how it was possible to shake off the shackles imposed upon us by the verb *to be* and to construct a very simple language which makes no reference to properties, classes, relations or other abstract entities.[1] At the same time I pointed out that in this language (which was, and will continue to be, referred to as WL) no provision had so far been made for dealing with words which would ordinarily be said to name sets of sets; and I also pointed out that I had not solved the problem of introducing or applying mathematics in the language WL. It is the object of the present paper to make some suggestions towards removing these limitations of the language WL.

In considering the relation of mathematics to biology we must distinguish between the process of applying existing mathematics to biology, and the less familiar process of letting biological statements suggest new mathematical ones. In connection with the first process it must be remembered that much of existing mathematics was suggested by, and was developed to meet the need of, physics. If we want to discover what sort of mathematics is needed for biology

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[1] The Arabic provides an example of a natural language which has no verb *to be*. If an Arabic-speaking person wishes to say that God is great, he concatenates the Arabic word for God with the Arabic word for great, just as in WL.

we must pay some attention to the second process.[1] I shall deal with this first, and, in order that everything should be as clear and easily understood as I can make it, I shall take very simple and familiar examples of statements concerning everyday life.

In ordinary life we are familiar with statements like 'Tom is father of Dick,' 'George is father of Mary,' and so on. We also notice that Dick has no father other than Tom, Mary no father other than George, and so on. We may reach the belief that this is the case for everyone. If we wish to give verbal expression to this belief we must drop all reference to Dick, George, etc., and say :

No one has more than one father

or :

If anyone is father of someone, then no one else is.

Such formulations suffice for the purposes of ordinary life, but in scientific discourse, which consists almost exclusively of such universal general statements, we very soon become confused over the various 'anyones,' 'someones' and 'no ones' involved. We are then driven to borrow from mathematics the device of using dummy names or variables in the place of 'Tom,' 'Dick,' etc., in order to secure unambiguous cross-reference ; at the same time prefixing to the resulting expressions such phrases as 'for all . . .' or 'for some . . .,' where, in the place of the dots, there will be the meaningless letters which we are using as variables. With these devices the above generalisation becomes :

'for all x, y and z, if x is father of z and y is father of z, then $x = y$.' (1)

Now although we have adopted the mathematical device of putting variables in the place of constants (in this case personal names) the result is not a mathematical statement, but a biological one (if 'father' is understood in its biological sense). It is not a mathematical statement because it contains the phrase 'is father of' which does not belong to mathematics.

[1] A. N. Whitehead, for example, pointed out that 'numerical relations derived from measurements constitute the subject-matter of a very special development of mathematics. This development has as yet constituted the only really important part of mathematics, except for the impressive disclosure of the extent to which ordinary Geometry is independent of measurement and number.

It follows that there are an indefinite number of purely abstract sciences, with their laws, their regularities, and their complexities of theorems—all as yet undeveloped. We can hardly avoid the conclusion that Nature in her procedures illustrates many such sciences.'

It is a further matter of everyday experience that, if Tom is father of Dick, then Tom and Dick must be distinct objects. Similarly, if George is father of Mary, George cannot be identical with Mary, quite apart from the fact that they are of different sexes. If we wish to generalise from these observations we can say :

No one is his (or her) own father

or

Father and offspring are always distinct

or

for all x and y, if x is father of y, then $x \neq y$. (2)

We thus have two universal general statements about fathers and children which give expression to the results of observations which we all make in everyday life. From these two statements it is possible to obtain a third, *without any further reference to observation*, namely the statement :

No one is both father and father's father of the same person

or

for all x, y and z, if x is father of y and y is father of z, then x is *not* father of z (3)

For suppose we have : (for any x, y and z) x is father of y and y is father of z and x is father of z. From the first of these statements it follows by our second generalisation that

$$x \neq y$$

and from the second and third of these statements it follows by our first generalisation that

$$x = y.$$

The conjunction of the above three statements thus leads to a contradiction, and if therefore we accept the first two we must reject the third, and this is expressed in statement (3).

Now let us leave these biological considerations. In a very different type of situation, which still does not take us away from everyday life, we notice that Paris is capital of France and Rome is capital of Italy, and that France has no capital other than Paris and Italy no capital other than Rome. If we wish to express the generalisation of these statements with the help of the device we adopted in the case of fathers and children, we can write :

for all x, y and z, if x is capital of z and y is capital of z, then $x = y$. (4)

We also notice that France is distinct from Paris its capital and Italy is distinct from its capital Rome. This can be generalised as:

for all x and y, if x is capital of y then $x \neq y$. (5)

Turning finally to yet another topic, we find that Bill is skipper of the *Nancy* and no one else is; and that George is skipper of the *Mary Anne* and no one else is. If we wish to construct a universal general statement on this basis we can say:

for all x, y and z, if x is skipper of z and y is skipper of z, then $x = y$. (6)

Again, we notice that the *Nancy* and her skipper are distinct objects, and that the same holds for the *Mary Anne* and her skipper. This generalises into:

for all x and y, if x is skipper of y, then $x \neq y$. (7)

If we now look over the six generalisations which we have reached in the course of everyday life, we see that we can divide them into two groups of three each. In each group two generalisations will differ from one another in only a single word. Thus (1) differs from (4) only in having 'father' where the latter has 'capital'; and (4) differs from (6) only in having 'capital' where the latter has 'skipper.' In just the same way (2) differs from (5) and (5) from (7).

We have now reached a critical point in our considerations. Just as there was a strong resemblance between 'Tom is father of Dick and no one else is' and 'George is father of Mary and no one else is,' which is brought into prominence when the differences between these statements are obliterated by replacing the personal names by variables, in just the same way there is a strong resemblance between generalisations (1), (4) and (6) which is made evident if we replace 'is father of,' 'is capital of' and 'is skipper of' by a variable, say the letter 'P' and obtain:

for all x, y and z, if $x\mathrm{P}z$ and $y\mathrm{P}z$, then $x = y$. (8)

By a corresponding replacement in (2), (5) and (7) we get:

for all x and y, if $x\mathrm{P}y$ then $x \neq y$. (9)

Now although (8) and (9) have been reached by a continuation of the process by which (1) and (2) were reached—namely, by the replacement of constants by variables—yet there are fundamental differences between the two cases. Perhaps the most important difference is that (8) and (9) show the highest possible degree of abstraction in the sense that they no longer contain a name, because 'if . . . then,' 'and,' and '$=$' are not names, but operation signs

which are common to all the sciences. To that extent (8) and (9) are candidates for admission into mathematics, although not as statements.

A second important difference is that in the case of statements (1) to (7) it is clear what can be substituted for '*x*,' '*y*' and '*z*,' in order that we may obtain a meaningful sentence after dropping the quantifying prefix. But when we ask the corresponding question regarding (8) and (9) the answer is not at all so clear. What can be substituted for '*x*,' '*y*' and '*z*' in (8) and (9) will obviously depend upon what can be substituted for 'P.' This brings us to the crux of our problem, because it is here—at the place where we reach the maximum degree of abstractness—that appeal is made to abstract entities. It is customary to say that 'P' can be replaced only by the names of two-termed relations, these relations being called the *values* of 'P.' Having taken this step we are then tempted to say that, when Tom is father of Dick, in addition to Tom and Dick a third entity—fatherhood—is necessary to 'relate' them and, because no such thing can be demonstrated, it is 'abstract.' If we try to escape from this by saying that 'P' can be replaced by names of sets of ordered couples, we still do not avoid abstract entities, because sets are abstract entities and so are ordered couples.

So long as it is felt and insisted that there must *be* something which is named by the signs which can be substituted for 'P' there seems to be no escape from abstract entities. This crucial point therefore merits further consideration. Let us call the signs which can replace a variable in an expression, and convert it into a name or a meaningful sentence, the *substitutes* [1] of that variable. Thus 'Tom' and 'Dick' are substitutes of '*x*' and '*y*' in '*x* is father of *y*.' It is customary to call the objects named by such substitutes the *values* of the variable, so that Tom and Dick are values of '*x*' and '*y*' in the above example. If the substitutes of a variable are names then the variable will have values; and if a variable has values then its substitutes will be names. But there is another possibility, when a variable has substitutes but these are not names, and so the variable has no values. This will be

[1] Professor Quine apparently wishes to coin the new word 'substituend' for this purpose. This seems to be a new word; at least it is not to be found in the Shorter Oxford English Dictionary. When we have the word 'substitute' in its substantival sense there seems to be no need at all for a new one. See W. V. Quine: 'Designation and Existence,' in *Readings in Philosophical Analysis*, edited by Feigl and Sellars, New York, 1949, p. 50.

the case if the substitutes of the variable are not names but *functors*.[1] Functors are syncategorematic signs, i.e. they have no meaning in isolation. They may be thought of as being accompanied by one or more blanks and they only yield a meaningful expression when the blanks are filled by names ; just as an application form or a blank cheque only yield an application or a cheque when their blanks are appropriately filled in. There are two kinds of functors : name-forming and statement-forming, according to the nature of the result of filling in the blanks. A name-forming functor may be said to be of degree *n* if it has exactly *n* blanks. In ' Science without Properties ' words like ' father ' were allowed to enter WL as *names*, and from these, with the help of brackets, name-forming functors like ' father ( ) ' (corresponding to ' father of . . . ') were defined for the purpose of constructing relational statements. For reasons explained in the footnote to page 201 of that paper, it would be better to let words like ' father ' enter WL not as names but as functors. (With this modification WL would contain three kinds of sign : names, operation-signs and functors.) On the present occasion let us suppose that we introduce them into WL as name-forming functors of degree 1. For example, we could use ' F . . . ' as an abbreviation for ' father of . . . .' When the blank is filled with a name the result will be a name which will name everything which is father of the object(s) named by the name inserted in the blank.[2]

If the procedure just outlined is adopted we can offer a solution of the problem presented by ' P ' in formulas (8) and (9). We can say that this variable has substitutes but no values, because its substitutes are name-forming functors of degree 1. But this still leaves a problem, namely the problem of what we are to understand by the *quantification* with respect to ' P ' of formulas (8) and (9), because these formulas are not statements. In order to obtain statements from them we must either replace ' P ' everywhere by one of its substitutes or we must write a quantifying prefix containing ' P '

[1] We owe this term to Professor T. Kotarbiński ; see Tarski, A. *Der Wahrheitsbegriff in den formalisierten Sprachen*, Leopoli, 1935, p. 274. In Alonzo Church's entry under this head in the *Dictionary of Philosophy* edited by D. D. Runes, New York, 1942, the term functor is connected only with the name of R. Carnap, but Carnap uses it in a very restricted sense ; see his *Logical Syntax of Language*, London 1937, p. 14.

[2] Note that this would render bracket names (described on page 200 of ' Science without Properties ') unnecessary.

in front of each formula. It is customary to explain such a statement as :

$$(x)\ (x = x)$$

by the words : ' everything is identical with itself' or ' for all x, x is identical with x ' or ' for all values of " x," x is identical with x.' When the variable concerned has functors and not names as its substitutes such an explanation of quantification is clearly ruled out: we cannot say ' for all P ' or ' for all values of " P " ' because there are no Ps. But we can perfectly well say : ' for all substitutes of " P." ' Against this it might be objected that this would make our statement a metalinguistic statement. It might be said that because ' P ' in the quantifying prefix is (and must be) in quotes, therefore the resulting statement will be about a linguistic entity and therefore metalinguistic. But this is no more the case than it is if I interpret the quantifier by the words ' for all values of " x." ' This is because what is asserted is the totality of statements resulting from the replacement of the variable by its substitutes, and this is the case whether those substitutes are names or functors. It seems then that we can safely quantify variables whose substitutes are functors, and not names, without being ' committed ' to recognising abstract entities, provided we are willing to make a slight change in our verbal explanation of quantification and provided this does not bring with it any change in the formal postulates governing the process of quantification.

The statement obtained from formula (8) by universally quantifying it in the above way, although it appears to satisfy the definition of a mathematical statement implicit in Lord Russell's definition of ' mathematics,'[1] yet it can easily be shown not to be one because it can be falsified by appeal to observation. If we replace the ' P ' of (8) everywhere by ' is brother of' we obtain :

for all x, y and z, if x is brother of z and y is brother of z, then $x = y$

and this is falsified by every instance of someone with more than one brother.

In the theory of relations formula (8) is used to define the class of one-many relations, and (9) to define the class of irreflexive relations. If we replace ' is father of' in the statement (3) by the variable ' P ' we obtain :

$$\text{for all } x, y \text{ and } z, \text{ if } xPy \text{ and } yPz \text{ then not } (xPz) \qquad (10)$$

[1] *The Principles of Mathematics*, London, 1942, p. 3

and this serves to define the set of intransitive relations. In WL such formulations are excluded but others can be devised which have the same or an analogous use. For example, we could concatenate 'P' with 'one-many' to form :

P one-many

and explain in a metalinguistic rule that this expression will yield a true statement if, and only if, when the same substitute (a name forming functor of degree 1) replaces 'P' in the above and in the following

$$(x)(y)(z)\ (\text{if } xPz \text{ and } yPz \text{ then } x = y)$$

the latter yields a statement which is accepted as true.

We now have all we require for formulating a specimen of a true mathematical statement. I will formulate it first in the language of the theory of relations :

Every one-many and irreflexive relation is intransitive. (A)

In the language WL, using the mode of abbreviation suggested above, this would be stated as follows : For every substitute of 'P,' if P one-many and P irreflexive then P intransitive. (B)

Alternatively we could formulate it in our metalanguage as follows : Every name-forming functor which satisfies the one-many formula and the irreflexivity formula also satisfies the intransitivity formula. (C)

We must now show that this is a true mathematical statement. In order to do this we must, according to (A), begin by saying: "Let Q be any one-many and irreflexive relation." According to (B) or (C) we must say "Let 'Q' be any name-forming functor which satisfies the requirements of the antecedent." *In both cases* we shall, on expanding these preliminary phrases in accordance with the definitions, obtain the following formulas, (1) and (2), which I write down as the premises in a scheme of natural deduction after the method of Quine[1] (and using '(x)' as an abbreviation for 'for all x,' etc.) :

* (1) $(x)\ (y)\ (z)\ (\text{if } xQz \text{ and } yQz \text{ then } x = y)$
* (2) $(x)\ (y)\ (\text{if } xQy \text{ then } x \neq y)$
* (3) if $x'Qy'$ then $x' \neq y'$ [from (2) by U.I.

[1] *Journal of Symbolic Logic*, Baltimore, 1950, **15**, 93-102. The starred numerals on the right in the scheme which follows refer to the numbered propositions in Whitehead and Russell's *Principia Mathematica*, 2nd ed., Cambridge, 1925.

★ (4) if $x' = y'$ then not $(x'Qy')$ [from (3) by transp. ★ 2.03
★ (5) if $x'Qz'$ and $y'Qz'$ then $x' = y'$ [from (1) by U.I.
★ (6) if $x'Qz'$ and $y'Qz'$ then not $(x'Qy')$ [from (5) and (4) by Syll. ★ 3.33
★ (7) if $y'Qz'$ and $x'Qz'$ then not $(x'Qy')$ [from (6) by ★ 4.3
★ (8) if $y'Qz'$ and $x'Qy'$ then not $(x'Qz')$ [from (7) by transp. ★ 3.37
★ (9) if $x'Qy'$ and $y'Qz'$ then not $(x'Qz')$ [from (8) by ★ 4.3
★ (10) (x) (y) (z) (if xQy and yQz then not (xQz)) [from (9) by U.G.x', y', z'.

We have thus shown that the intransitivity formula is obtainable from the two initial formulas merely by subjecting them to a succession of purely structural transformations which are well known to be truth preserving, i.e. such that when applied to true statements will yield true statements. In Carnap's terminology our statement, in either of the forms (A), (B) or (C), has been shown to be L-true.

It is easy to see that this result is not in any way dependent on any particular interpretation of 'Q.' All that is assumed about it is that the expressions 'xQy,' 'yQz' and 'xQz' behave (as far as the above-mentioned transformations are concerned) like sentences, and would *be* sentences if 'Q' were replaced by any name-forming functor of degree 1, and 'x,' 'y' and 'z' by corresponding sense-making *names*. In consequence of this we can now see that the possibility of obtaining the statement (3) above depended only on the fact that 'is father of' satisfies the one-many and irreflexivity formulas and not at all on the fact that the statements were about fathers and children. Our mathematical statement applies impartially to a great variety of topics—to ships and their skippers, countries and their capitals and so on. For example, suppose we let 'xDy' stand as an abbreviation for 'x is a cell which has divided into two or more cells of which y is one.' Then clearly:

for all x, y and z, if xDz and yDz then $x = y$

and

for all x and y, if xDy then $x \neq y$

so that 'D' satisfies the one-many and irreflexity formulas; and so, according to the mathematical principle which we have established, we must also have:

for all x, y and z, if xDy and yDz then not (xDz).

Our first example of a mathematical statement illustrates the following points: (i) it has the form of an if . . . then . . .

statement ; (ii) it contains only variables and logical constants ; (these are the points referred to in the definition given by Lord Russell which has already been mentioned) ; (iii) it can be shown to be true without any appeal to observation, applied to the subject matter of any empirical science ; it is L-true ; (iv) whatever may have suggested it in the first instance, it belongs to no branch of natural science (on account of (ii)) but is applicable to any subject-matter which is expressed with the help of name-forming functions of degree 1 ; (v) it was not reached, like the statements of natural science, by induction from records of observations, but its constituents were reached from empirical statements (belonging to a wide range of topics) by successive replacements of constants by variables until only logical constants were left. The combination of the three constituents (i.e. formulas (8), (9) and (10)) into a single statement, took place when it was noticed that the third was a logical consequence of the conjunction of the first two. (It is not suggested that *all* mathematical statements are arrived at in this way.) (vi) If we adopt formulation (B) or (C) of our example of a mathematical statement, then there is no special mystery either about the way in which the materials for constructing such statements can arise from the statements of natural science, or about the way in which they are applicable to fresh contexts in natural science ; (vii) Mathematical statements do more than provide a means of working out the consequences of existing hypotheses in natural science. One of the great merits of going to the extreme of abstraction by turning all names and functors into variables is that the imagination is set free and combinations of ideas may suggest themselves which would not otherwise be thought of. This provides an incentive to looking for exemplifications of them in the observable world. This, I think, is what A. N. Whitehead had in mind when he wrote :

> The true method of discovery is like the flight of an aeroplane. It starts from the ground of particular observation ; it makes a flight in the thin air of imaginative generalisation ; and it again lands for renewed observation rendered acute by rational interpretation.[1]

[1] *Process and Reality*, Cambridge, 1929, p. 5. The following is an additional argument in favour of the process of replacing constants by variables until the extreme of abstraction is reached : (i) An explanatory hypothesis in natural science must have empirical generalisations among its consequences ; (ii) the consequence relation depends only on the structure of the statements concerned ; (iii) the search for explanatory hypotheses should accordingly be facilitated by the use of a language which makes clear the structure of the empirical generalisations concerned. This is done much better by the mathematical languages than by the natural ones.

(viii) Finally, it is I think worth pointing out that the abstract entities of mathematics serve only to provide its names with nominata (unless we view them not as names but as functors so that no nominata are required), and appear to play no part in decisions regarding the truth of its statements; whereas the concrete objects of natural science not only supply nominata for its names but observations of some of them are appealed to for deciding the truth of its statements.

I propose to give now some simple examples that have occurred to me of the process of obtaining abstract notions from empirical ones. At one time I found myself applying the term 'hierarchy' to very diverse objects and without being able to state at all in words why I did so. For example: draw a square on a piece of paper, bisect each side and join the middle points of opposite sides to produce four smaller squares. If 'X' names each of these smaller squares then 'ΣX' names the larger square of which they are parts. This process can now be applied to each small square, producing sixteen still smaller squares, and so on. The resulting system of squares within squares exemplifies the notion of hierarchy as I understand it. Or, consider a zygote which divides into two cells each of which also divides into two, and so on. This time-extended system of cells also exemplifies the notion of hierarchy. Finally, consider the taxonomic categories in a system of Linnean classification of animals or plants. We begin with a greatest class which is divided into a number of mutually exclusive major subclasses; each of these latter is then divided in a similar way, and so on. This system of taxonomic categories also exemplifies the notion of hierarchy. After drawing up definitions which were satisfied by the particular examples it became possible finally to replace all the empirical constants in these definitions by variables and so to reach a purely abstract definition of hierarchy. Such a definition is given on page 42 of my *Axiomatic Method in Biology*. The following is a still simpler definition (stated, for the sake of brevity, in the language of the theory of relations):

> R is a hierarchy if and only if R is one-many and if the converse domain of R is identical with the set of all terms to which the first term of R stands in some power of R.[1]

[1] I am indebted to Professors Quine and Myhill for the improvements incorporated in this definition. It does not suffice to define 'hierarchy' as denoting the set of all one-many relations which have one and only one beginner. In the first of the above illustrations of the notion of hierarchy the generating relation is the relation in which a square stands to each of its four quarter squares; in the

Having obtained such a definition it is then easy to think of and to define various theoretically possible types of hierarchy and to formulate laws connecting them. It is also easy to see how hierarchies are related to other mathematical notions. For example it is at once evident that a progression, as defined in *Principia Mathematica*, is a special case of an infinite hierarchy, in which the requirement that the generating relation should be one-many is strengthened by the further requirement that it should be many-one (and thus one-one). Again, by the use of proposition $\star$ 96.24 of *Principia Mathematica* we can see that if R is a hierarchy not only is R irreflexive but R_{po} must also be irreflexive.

If we confine attention to organisms in which there are just two sexes—male and female—which are mutually exclusive (no hermaphroditism) we can define the *pedigree* of any individual x as that subrelation of the parental relation which results when the field of the latter is limited to x and all its ancestors (so long as these ancestors satisfy the above condition regarding sex); x will be the last term of this relation and the sole member of the zero generation of the pedigree; its two parents will constitute the first generation, and so on. We can say that a pedigree is regular up to the nth generation if, and only if, for any i such that $0 \leqslant i \leqslant n$, the number of terms in the ith generation is equal to 2^i. The question now suggests itself: can we find a formula which will 'number off' the terms of a pedigree, beginning with x, its last term; in other words, can we define a finite sequence which correlates the terms of the pedigree with a definite subset of the natural numbers? An affirmative answer is rendered possible by the fact that each member of the converse domain of a pedigree has just two parents, one of which is male and the other female, and that male and female are mutually exclusive. Let us use '$N(y)$' to denote the number assigned to any term y, and let us denote the male parent of y by '$F(y)$' and the female parent by '$M(y)$.' Then for any pedigree of which the last term is x the required formula is established by:

(i) $$N(x) = 0$$

(ii) for any term y, if $N(y) = m$, then
$$N(F(y)) = 2m + 1$$
$$N(M(y)) = 2m + 2.$$

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second it is the relation D explained above (p. 9), and in the third it is the converse of the relation of immediate inclusion of classes.

In this way every term receives not only a designation constructed by repetitions of 'F' and 'M,' but also a numeral; if the pedigree is not regular, some terms will receive more than one numeral. We therefore have a device by which some statements concerning pedigrees can be formulated in arithmetical terms. It also provides a clue for applying the same procedure to certain hierarchies, because if P is a pedigree which is regular up to the kth generation, with x as its last term, then its converse will satisfy the definition of a regular[1] hierarchy with $k + 1$ levels[1] of which x is the first term and the sole member of zero level. In this case we have what I call a $1 \rightarrow 2$ hierarchy because each member of its domain stands in the generating relation to just two terms. But we need not restrict attention to 2 but can deal quite generally with regular $1 \rightarrow k$ hierarchies, where k is any natural number greater than 1. Now it is clear from the hint provided by pedigrees what conditions must be satisfied if we are to be able to give a formula which will correlate the members of the field of a regular $1 \rightarrow k$ hierarchy having x as its first term with a subset of the natural numbers. This can be most briefly expressed if we first define *assemblage* of a hierarchy. If y is any term belonging to the domain of a hierarchy R, then I call the set of all terms to which y stands in R an assemblage of R. Now if we are to number the terms of a finite regular $1 \rightarrow k$ hierarchy by means of a formula it is necessary and sufficient that we should have k pair-wise mutually exclusive sets:

$$X_1, X_2, \ldots, X_k$$

such that each assemblage of the hierarchy in question is a *selection* from these k sets. That is to say, each member of each assemblage (and there will be k members in each) must belong to one (and can only belong to one) of these k sets and no two members of the same assemblage may belong to the *same* one of the k sets. The required equations which assign a numeral to each term of the hierarchy are then easily formulated in a way closely analogous to that followed in the case of pedigrees.

In conclusion of this part of my discussion let me summarise the main points. In a language which possesses, in addition to names and signs of operations, name-forming functors, the temptation to talk about relations will arise as soon as the process of replacing

[1] These terms are defined in J. H. Woodger, *The Axiomatic Method in Biology*, Cambridge, 1937, p. 44.

constants by variables reaches the point where it involves name-forming functors. If talk of abstract entities is to be avoided it seems to be necessary to distinguish between quantification of variables which have values as well as substitutes, and quantification of those which only have substitutes. At the same time, if this should prove to be a satisfactory way of eliminating talk about classes and relations, we can return to the latter practice (as I have done above in speaking about hierarchies) with a clear conscience, not feeling that we are 'committed' to abstract entities, but enjoying the benefits of brevity and familiarity which are associated with the traditional formulations.

2

In this section the problem of introducing numerals into the language WL will be discussed.

According to the class theory, to say that there are nine planets is to say that the set of planets has nine members. 'Nine' is thus regarded as denoting the set of all sets which have exactly nine members; planet is thus a member of nine. But as we wish to avoid the notion of class or set this method is not available in WL.

Various possibilities suggest themselves. By analogy with the procedure of the previous section we might introduce natural number signs into WL as statement-forming (not name-forming) functors of degree 1, e.g. 'nine . . . .' We should then require a semantical rule distinguishing the result of filling the blank with a name from the concatenation of two names, and explaining that the resulting statement would be true if, and only if, the number of objects named by the name was exactly as indicated by the definition of the functor. The functors required could be progressively defined as follows:

(i) $0X$ if and only if $X\Lambda$
(ii) $1X$ if and only if $X = \Sigma X$[1]
(iii) $2X$ if and only if for some Y and some Z, $1Y$ and $1Z$ and $X = Y \cup Z$,

and so on. In this way we should have signs functioning as numerals but no numbers, just as in the previous section we had signs function-

[1] The significance of names constructed with the help of the Greek letter sigma is explained in Rule 12 of WL, see 'Science without Properties,' this *Journal*, 1951, **2**, 203.

ing as though they were relation names but no relations. But there is another method suggested by a consideration of the counting process which may be of interest.

Suppose a sheep farmer in Australia wishes to communicate with a friend in England about the colour, length or weight of his sheep. The two men must in each case agree on some common scale or basis of matching to which they can both refer. In each case too, two procedures are available: a verbal or linguistic one and a more primitive and direct one. For example, if the Australian wishes to tell his friend that one of his sheep is a yard long he can either simply use the word ' yard ' hoping that his friend will understand it to refer to the same standard that he has in mind himself, or he can cut off a piece of string one yard in length and send this through the post with a note to the effect that his sheep has the same length as the piece of string. Similarly in the case of colour: he can either say that his sheep are dark brown or he can send a piece of cloth or paper of the required colour with an explanatory note. Theoretically the two methods are also available in the case of weight, although the second method would be expensive and if adopted generally would be unpopular with the postal authorities.

Corresponding remarks also apply to the *number* of the sheep. Our Australian can write to his friend saying he has fifty sheep, relying on his friend's knowing what he means by ' fifty '; or he can actually make a specimen of the standard by taking a piece of paper, making fifty strokes (or dots, or crosses, or . . . , etc.) on it and adding the information that he has as many sheep as there are strokes (or dots, etc.) on the paper.

In order to obtain the information to be transmitted, the farmer must perform certain matching operations: matching of colours, lengths, etc. This again also applies to discovering the number of sheep in his flock: he must perform a numerical matching or counting.

The process of counting requires the successive performance of *two* operations. The first operation is one which is performed on the object to be counted. The second operation is performed upon what we can call the counting apparatus. After the first operation has been performed the second is immediately carried out, and then, if possible, the first is repeated and again followed by the second. Such repetitions are continued until it is no longer possible to perform the first operation and the whole process then automatically ends, because the second operation can only be performed immediately

after the first has been performed. That part of the counting apparatus which shows the effects of the second operation may be called the *record* of the process, and each single effect of the second operation will be called an *element* of the record.

Suppose, for example, the object to be counted is a flock of sheep, and it is to be counted with respect to the sheep in it.[1] Suppose further that the counting apparatus consists of a smooth stick and a knife. A field of suitable size is first cleared of sheep and any gaps in the fence or open gates are closed. A gate is then opened and through it the flock is driven into the field. The *first* operation would then consist in driving *a single sheep* out again into an adjoining field. The *second* operation would consist in cutting with the knife a single notch in some part of the stick which is devoid of notches. Repetitions of the two operations now continue until no more sheep remain in the field. The first operation can then no longer be performed and the process is complete. The stick with its notches now constitutes the record, with each notch as an element in the record.

The same two operations can be discerned in the more sophisticated counting processes. By means of the apparatus called a turnstile, fixed at the entrance of an exhibition or a sports ground, the two operations are performed automatically. Here the object to be counted is the 'gate,' and this is counted with respect to the persons admitted. The counting apparatus is the recording machinery geared to the axle of the turnstile. The first operation is performed when a person pushes his way through the turnstile and so rotates the axle. The rotation of the axle operates the recording apparatus and so performs the second operation. The record in this case is a numeral printed or engraved on the apparatus.

If an object X has been counted with respect to those of its parts which are named by 'Y' and if R is the resulting record and 'E' names each of its elements, then we shall say that R with respect to E matches numerically X with respect to Y. We shall assume that, for any X, Y, Z and W, U and V:

(i) if X with respect to Y matches numerically Z with respect to W, then Z with respect to W matches numerically X with respect to Y;

[1] Note that this formulation does not involve abstract entities. If 'S' names every sheep possessed by Farmer George, then 'ΣS' names his flock. ΣS is counted with respect to S. It could also be counted with respect to E, where 'E' names every eye, or with respect to L where 'L' names every leg, and so on.

(ii) if X with respect to Y matches numerically Z with respect to W and Z with respect to W matches numerically U with respect to V, then X with respect to Y matches numerically U with respect to V.

Leaving aside now the problem of counting we turn to the linguistic apparatus which is required for giving verbal or symbolic expression to the result of a counting process. The signs required will be called *numerals*. These cannot be regarded as names (e.g. as names of numbers) if we are rejecting abstract entities. Numerals must accordingly be introduced into WL as *statement-forming functors* of degree one. I proceed first to define 'numeral of WL' in the metalanguage.

Rule 13. (i) a vertical stroke 'I' is a primitive numeral in WL; (ii) if N and M are any primitive numerals in WL, then both of their concatenations are also primitive numerals in WL and each can be called *a successor* of both M and N. In particular, if M is 'I,' then the concatenation of N with M will be called *the immediate* successor of N; (iii) A numeral in WL is either a primitive numeral of WL, or a sign introduced by definition as an abbreviation for a primitive numeral of WL (e.g. '5' is an abbreviation for 'IIIII'), or a sign defined with the help of primitive numerals (or their abbreviations) and signs of operations on such numerals.

These are the primary numerals which are required for recording and communication in connection with counting processes. For recording the results of measuring processes rational number-signs are also required, and these can be defined with the help of the primary ones and operations upon them. All other numerals appear to owe their introduction either to the requirements of calculation (e.g. in order that certain operations with numerals shall always be performable) or to the demands of certain of the postulates which occur in scientific theories. Thus the sign '0' cannot occur in connection with counting because the first operation cannot be performed in counting something with respect to what is named by a fictitious name. Similarly '$\aleph_0$' cannot occur in connection with counting, not because the process can never begin, but because it can never end in such connections. Nevertheless, it is not uncommon to call sets with $\aleph_0$ members *countable* sets. Clearly the word 'countable' in such contexts must mean something quite different from the process described above. The demand for transfinite numerals comes

chiefly from the introduction of the notion of precedence in time into a language, because the postulates by which it is introduced usually assume that this relation has Dedekindian continuity.

Statements in WL formed by means of numerals will be of two principal kinds: (i) those formed by concatenation with a name to the left of the numeral, and (ii) those formed by numerals (or numeral-forming functors, e.g. '$Te_u(X)$' for 'the temperature of X in unit u')[1] and the sign of identity or inequality.

Rule 14. (i) If N is a name in WL and M a numeral, then the concatentation N⌢M is a statement of WL; (ii) if N and M are names in WL and K is a numeral, then N⌢M⌢K is a statement in WL; (iii) if N and M are two numerals or two numeral-forming functors or one is a numeral and the other a numeral-forming functor, then the concatenation of N to the left and M to the right of the sign of identity or the sign of inequality is a statement of WL.

Rule 15. (i) If S is any statement of WL which is formed in accordance with Rule 14 (i), then S is true in WL if, and only if, the object named by sigma N, with respect to the objects named by N, matches numerically the numeral M with respect to its strokes, or (if M is a defined numeral), the primitive numeral of which M is an abbreviation with respect to its strokes; (ii) if S = N⌢M⌢K is any statement of WL which is formed in accordance with Rule 14 (ii), then S is true in WL if and only if the object named by N with respect to the objects named by M matches numerically the numeral K with respect to its strokes, or (if K is a defined numeral), the primitive numeral of which K is an abbreviation, with respect to its strokes; (iii) if S is any statement of WL which is formed in accordance with Rule 14 (iii), then S is true in WL if N and M are mutually substitutable in any statement of type (i) without altering its truth value (if S contains the sign of identity) or if N with respect to its strokes matches numerically a proper part of M with respect to its strokes (if S contains the sign '$<$').

Thus if 'S' names each sheep of the flock referred to above, and if the stick with respect to its notches, which was the record of the operation of counting that flock with respect to its sheep, matches

[1] 'Te_u ( )' is an example of a functor in the sense in which R. Carnap uses that term; and if numerals are names it is a name-forming functor. But if numerals are *not* names then it cannot be a name-forming functor or a functor *at all* in our sense. Consequently it should not strictly speaking be called a numeral-forming factor until the meaning of the term 'functor' has been extended.

numerically IIIIIIIII with respect to its strokes, and if ' 10 ' is defined as an abbreviation for ' IIIIIIIII ', then ' S 10 ' is true.

I must leave it to experts to define addition and multiplication [1] on this basis, and to show that the operations so defined obey the usual laws, i.e. that they are commutative and associative and that multiplication is distributive with respect to addition. Should this prove feasible WL would then have been equipped with an arithmetic which (assuming rational-number signs to have been defined) will suffice for most if not all biological purposes, but which at the same time does not require reference to abstract entities of any kind.

3

There remains the problem of how to deal with words which would ordinarily be regarded as naming sets of sets. I shall not attempt to deal with this problem quite generally but will discuss it only in connection with the example mentioned in ' Science without Properties,' namely the word ' species.' If ' *Homo sapiens* ' is regarded as a name for a set of organisms, then ' species ' must be a name for a set of sets of organisms.[2] But do biologists regard species as sets?

In the Linnean system of classification of animals and plants a species was a set or class, in fact it originally meant a smallest named class in the system. But a class or set is an abstract entity and thus has neither beginning nor end in time. We cannot, therefore, speak of the origin of species if we are conceiving species in the Linnean manner. The doctrine of evolution is not something that can be grafted, so to speak, onto the Linnean system of classification. The species of Darwin and the species of Linneus are not at all the same thing—the former are concrete entities with a beginning in time and the latter are abstract and timeless. And if this is so, it is at once clear how species-names in the evolutionary sense are to be constructed in WL. Thus, if ' *Homo sapiens* ' names every man, woman and child (in accordance with the treatment of general names in WL), then ' Σ *Homo sapiens* ' names the concrete evolutionary species, and

[1] See, for example, R. Carnap, *Logical Syntax of Language*, London, 1937, p. 59, D1 to D6, or D. Hilbert and P. Bernays, *Grundlagen der Mathematik*, Berlin 1934, Vol. I, p. 22 et seq.

[2] See John R. Gregg, ' Taxonomy, Language and Reality,' *The American Naturalist*, 1950, **84**, 419–35.

the concatenation of this latter name with the name 'species' will, according to the rules of WL previously given, form a statement which is true if, and only if, everything named by '*Σ Homo sapiens*' (and there is only one such thing) is also named by 'species.' 'Species' in its evolutionary usage is thus a general name which names concrete entities, like all names in WL.

There is a feature which is inseparable from the evolutionary notion of species, but is absent from the Linnean, namely the notion of the parental relation. This feature is not essential to the question of species-names but it is intimately connected with the question of beginning in time. When reproduction is asexual so that each individual which is part of a species has only one parent, each (evolutionary) species would be the sum of the field of a hierarchy included in the parental relation. But when two distinct sexes are involved the problem is much more complicated and is one about which it is very unwise to be too definite. The difficulty can perhaps be explained in the following way. For the sake of brevity I will use the language of set-theory. Suppose, then, that Y is a set of organisms which satisfies the following conditions : (i) no parent of a member of Y belongs to Y ; (ii) if x is any member of Y then there is a y and a z such that y is parent of x and of z, and z does not belong to Y ; (iii) there exists at least one organism such that Y is a barrier[1] of its pedigree ; (iv) if x is any descendant of members of Y then a subset of Y is a barrier of its pedigree. Now let X consist of Y together with all the descendants of members of Y, then ΣX *could* be a species, the beginning of which would be the members of Y. But a species *need* not (theoretically) be related in precisely this way to X. There might be a proper subset of Z of X having no members in common with Y such that ΣZ is a species. In other words evolutionary species need not correspond exactly with the branching of phylogenetic trees, but could arise within existing parts of the system without branching.[2]

Discussions about the reality or existence of species, genera, etc., are sometimes found in biological works. Some biologists say that although species are real and exist, other taxonomic categories like genera, families, etc., are not real and do not exist. As a rule criteria of reality and existence are not given in such discussions, and it is therefore not always perfectly clear what is intended. If by species,

[1] See definition on p. 75 of my *Axiomatic Method in Biology*, 1937

[2] See my *Biology and Language*, Cambridge, 1952, p. 250

genera, etc., are meant taxonomic sets or classes, then the question resolves itself into the question whether abstract entities exist. In WL this difficulty has been avoided by excluding the notion of set or class and showing that their use represents a convenient *façon de parler* which can be avoided. But even if we admit abstract entities this provides no justification for distinguishing between species and other taxonomic sets. As abstract entities they are, so to speak, all in the same boat; they sink or swim together.

But it may be that in the discussions referred to the authors are conceiving species, genera, families, etc., as concrete entities whose names are sigma names, as explained above. These are the evolutionary species and genera. But in that case there is again no justification for distinguishing species as real from genera as unreal. Species will then be *parts* of genera. They will be distinguished only by their *size*. Only if we regard species exclusively in the evolutionary sense and genera exclusively in the taxonomic sense, can we say that species are real and genera unreal, and then only if we wish to deny the existence of abstract entities. But this is clearly an unsatisfactory mode of comparison. The taxonomic system and the evolutionary phylogenetic scheme are quite different things doing quite different jobs and only confusion will result from identifying or mixing them.[1]

Biology Department
The Middlesex Hospital Medical School
London W 1

[1] I am indebted to Professor Popper for discussing this paper with me in the course of its preparation. I am indebted to Mr Michael Woodger for pointing out an error in 'Science without Properties.' In the third line from the bottom of p. 201 (excluding the footnote) the last two words ('part of') should be deleted; and the first word of the next line ('something') should also be deleted.

IDEAL TYPES AND HISTORICAL EXPLANATION *

J. W. N. WATKINS

1 *Introduction*

IN this paper [1] I shall consider : first, what sort of creatures ideal types should be if they are to be used in the construction of social theories ; and secondly, what we do when we try to explain historical events by applying such theories to them.[2]

[1] Originally written to meet a problem which arose in the course of Professor K. R. Popper's seminar at the London School of Economics on 'The Philosophy of History.' I owe a very great debt to Professor Popper's teachings and criticisms. A much altered and expanded version was read to the Oxford Social Studies Association on 23rd November 1951, where it was subjected to some pertinent criticism from which the present version has, I hope, benefited.

[2] The formal structure of a prediction is, of course, the same as that of a full-fledged explanation. In both cases we have : (*a*) initial conditions ; (*b*) universal statements ; and (*c*) deductive consequences of (*a*) plus (*b*). We explain a given event (*c*) by detecting (*a*) and by postulating and applying (*b*) ; and we predict a future event (*c*) by inferring it from some given (*a*) and postulated (*b*). Nevertheless, I think that in social science explanation and prediction should be considered separately, for two reasons. First, as Professor G. C. Hempel has pointed out in a most illuminating discussion of this problem (see his 'The Function of General Laws in History' in *Readings in Philosophical Analysis*, ed. H. Feigl and W. Sellars, New York, 1949, pp. 462-5) in history we often have to be content (and in fact *are* content) with what he calls an explanation *sketch*, i.e. a somewhat vague and incomplete indication of (*a*) and (*b*) from which (*c*) is not *strictly* deducible. And if we go back to a time when (*a*) but not (*c*) has occurred, this partial sketch of (*a*) and (*b*) will not allow us to predict (*c*). For example, we may be satisfied by the explanation that Smith insulted Jones because Jones had angered him, although we should *not* be prepared to admit that if Jones angers Smith in the future, Smith will necessarily react by insulting Jones.

Secondly, even the social scientist who can provide a *full-fledged* explanation of a past event will run into difficulties if he tries to predict similar events, because they will occur in a system which is not isolated from the influence of factors which he cannot ascertain beforehand. The Astronomer Royal can prepare a Nautical Almanac for 1953 because he is predicting the movements of bodies in a system isolated from extraneous influences, but the Chancellor of the Exchequer cannot prepare an Economic Almanac for 1953 because, even if he possessed sufficient knowledge to explain completely the 1951 levels of prices, production, investment, exports, etc., his

* Received 15. i. 52

2 *Holistic and Individualistic Ideal Types*

It is only decent to begin a discussion of ideal types by considering Weber's views ; but he held two successive conceptions of what an ideal type should be and do, without, I think, realising what important differences lay between them.

His earlier version is set out in an article translated under the title '" Objectivity " in Social Science and Social Policy.' [1] At this time (1904) Weber believed that the social scientist should not try to imitate the natural scientist's procedure of systematically subsuming observation-statements and low order theories under more comprehensive laws. The social scientist should first decide from what point of view to approach history. Having decided, say, to treat its economic aspect, he should then select from this some unique configuration of activities and institutions, such as ' the rise of capitalism.' Then he should pin down and describe its components. His final task is to draw in the causal lines between these components, imputing ' concrete effects to concrete causes.' [2]

This programme could never be carried out ; ' in any actual economic system so many factors are at work simultaneously that the effect of a single factor by itself can never be known, for its traces are soon lost sight of.' [3] And separate facts cannot be linked together as causes and effects with no reference to general laws. However, I will not press these criticisms of a methodological position which Weber tacitly abandoned later.

To assist the social scientist in this task of explaining particular events by relating them to their particular antecedents, Weber proposed his first version of the ideal type. This was to be constructed by abstracting the outstanding features from some (more or less clearly demarcated) historical complex, and by organising these into a coherent word-picture. The ideality of such a type lies in its simplification and aloofness from detail : it will be free from the detailed complexity of

predictions of future levels would undoubtedly be upset by unforeseeable, world-wide disturbing factors, the effects of any of which might be cumulative.

Hence, the problem of social prediction raises questions not raised by the problem of historical explanation ; and this paper is not concerned with the former.

[1] Max Weber, *The Methodology of the Social Sciences*, trans. and ed. E. A. Shils and H. A. Finch, Illinois, 1949, ch. 2

[2] Op. cit. p. 79

[3] Walter Eucken, *The Foundations of Economics*, trans. T. W. Hutchison, London, 1950, p. 39

the actuality to be analysed with its aid. As this kind of ideal type emphasises the 'essential' traits of a situation considered *as a whole*, I call it 'holistic,' in contrast with the 'individualistic' ideal type described by Weber in Part I of his posthumous *Wirtschaft und Gesellschaft.*[1]

In this work he held that the social scientist's first task was to build up a generally applicable theoretical system ; and for arriving at this he proposed the use of ideal types similar to the models used in deductive economics. These are constructed, not by withdrawing from the detail of social life, but by formalising the results of a close analysis of some of its significant details considered in isolation. The holistic ideal type was supposed to give a bird's eye view of the broad characteristics of a whole social situation, whereas the individualistic ideal type is constructed by inspecting the situations of actual individuals, and by abstracting from these : (*a*) general schemes of personal preferences ; (*b*) the different kinds of knowledge of his own situation which the individual may possess ; and (*c*) various typical relationships between individuals and between the individual and his resources. An individualistic ideal type places hypothetical actors in some simplified situation. Its premisses are : the form (but not the specific content) of the actors' dispositions, the state of their information, and their relationships. And the deductive consequences of these premisses demonstrate some principle of social behaviour, e.g. oligopolistic behaviour. The ideality of *this* kind of ideal type lies : (i) in the simplification of the initial situation and in its isolation from disturbing factors ; (ii) in the abstract and formal, and yet explicit and precise character of the actors' schemes of preferences and states of information ; and (iii) in the actors' rational behaviour in the light of (ii). It is not claimed that a principle of social behaviour demonstrated by an individualistic ideal type will often have an exact empirical counterpart (though the principle of perfect competition has been precisely manifested, for instance in commodity-markets). But economists do claim that there is a limited number of basic economic principles, and that any economic phenomenon is a particular configuration of some of these, occurring at a particular place and time, which can be explained by a synthesis of the relevant ideal types, and by specifying the content of their formal premisses.[2]

[1] Translated by A. R. Henderson and Talcott Parsons as *The Theory of Social and Economic Organisation*, introd. by Talcott Parsons, London, 1947

[2] 'This morphological study of economic history reveals a *limited* number of pure forms out of which *all* economic systems past and present are made up.' Eucken,

Weber was no Platonist; he proposed both kinds of ideal type as heuristic aids which, by themselves, tell you nothing about the real world, but which throw into relief its deviations from themselves. The individualistic ideal type was to assist in the detection of disturbing factors, such as habit and tradition, which deflect actual individuals from a rational course of action—a proposal I shall examine later (see p. 41). Now I shall examine the assumptions underlying Weber's earlier proposal to use holistic ideal types.

One might improve one's appreciation of the shape of a roughly circular object by placing over it an accurate tracing of a circle. This analogy brings out Weber's conception of the purpose, and manner of employing, holistic ideal types in three respects. (i) By comparing an impure object with an ideal construction the deviations of the former from the latter are thrown into relief; and Weber did regard this kind of ideal type as a 'purely ideal *limiting* concept with which the real situation . . . is *compared* and surveyed for the explication of certain of its significant components.' [1] (ii) Both the object and the construct are considered *as a whole*. (iii) The analogy involves what is presupposed by the idea of comparison, namely, a simultaneous awareness of the characteristics of both things being compared. And in 1904 Weber did assume that the social scientist can place his knowledge of a real situation alongside his knowledge of an ideal type he has himself constructed, and compare the two.[2] It is the simultaneous

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op. cit., p. 10 (my italics). The 'de-idealisation' of the pure principles of economic theory which occurs when they are combined into a particular configuration which is applied to an empirical counterpart, is exactly paralleled in the natural sciences. For example, Galileo combined the Law of Inertia (which describes the motion of a body not acted upon by any force—a condition which can never be realised), and the Law of Gravity (which describes the motion of a body in a vacuum which the experimenter cannot obtain), and the principles of air resistance, into a theoretical configuration which allows complete prediction of the trajectories of e.g. cannon-balls, if the initial conditions are known. 'All universal physical concepts and laws . . . are arrived at by idealisation. They thereby assume that simple . . . form which makes it possible to reconstruct any facts, however complicated, by synthetic combination of these concepts and laws, thus making it possible to understand them.' (Ernst Mach, quoted by F. Kaufmann, *Methodology of the Social Sciences*, New York, 1944, p. 87.)

[1] *Methodology*, p. 93

[2] Thus he speaks of 'the relationship between the logical structure of the conceptual system . . . and what is immediately given in empirical reality' (op. cit., p. 96). The term 'immediately given' should not, I think, be taken too seriously. What this phrase does imply is that the social scientist's knowledge of ideal type and corresponding reality are on an equal footing.

knowability of the features of both which enables holistic ideal types to be 'used as conceptual instruments for *comparison* with and *measurement* of reality.' [1]

At this point an awkward question arises: If the characteristics of a historical situation have already been charted *before* the ideal type is brought into play, why bother with ideal types? They are not hypotheses [2] which guide the social scientist in his search for facts, for they are not supposed to be realistic, or empirical. A holistic ideal type is not a guess about reality, but an *a priori* word-picture—in other words, a definition. What Weber's earlier proposal amounts to is that holistic ideal types should be used as explicit definitions of those 'hundreds of words in the historian's vocabulary [which] are ambiguous constructs created to meet the unconsciously felt need for adequate expression and the meaning of which is only concretely felt but not clearly thought out.' [3]

Thus the holistic ideal type transpires to be something of a mouse, a mere demand for definitions; [4] and I shudder when I imagine each of those 'hundreds of words' being replaced by lengthy verbal definitions, though such defining *may* be helpful in particular circumstances. For instance, to order and classify a collection of variegated instances it may be necessary to construct a scale with limiting ideal types at either end. The survey of the constitutions of 158 Greek states was probably tidier and more systematic than it would have been if Aristotle's 'Monarchy-Aristocracy-Polity' and 'Tyranny-Oligarchy-Democracy' scales, or some equivalent, had not been used.

But such scales are for classifying facts already analysed, not for analysing raw material; and the real weakness of Weber's earlier proposal lies in the method of historical analysis which was to accompany the use of holistic ideal types. With *individualistic* ideal types, it will be remembered, we *start* with individuals' dispositions, information and relationships, and work outwards to the unintended consequences of their interaction (deducing a price-level, for example, from demand and supply schedules). But with *holistic* ideal types the analysis is supposed to proceed in the opposite direction. Here, the historian is supposed to start with the broad (or 'essential') characteristics of an entire historical situation, and then to *descend* to an ever closer definition of its deviations from the ideal type with which it is

[1] Op. cit. p. 97 [2] Op. cit. p. 90 [3] Op. cit. pp. 92-3

[4] For a criticism of such demands, see K. R. Popper, *The Open Society and Its Enemies*, London, 1945, vol. 2, ch. 11, sect. ii

being compared. In principle, this descent from overall traits to detailed ingredients might continue until, *at the end of the analysis*, the relevant dispositions, information, and relationships of the people concerned had been established.

The idea that we can apprehend the overall characteristics of a social situation *before* learning something of the individual situations of the actors in it *appears* to be borne out by a statement such as, 'The British economy in 1850 was competitive.' This statement apparently attributes an overall characteristic to a demarcated whole, while saying nothing about individuals (just as 'The lake's surface was calm' says nothing about water-particles). Now the unintended merit of the holistic ideal type is that its use forces us to recognise the falsity of this idea. If, in order to assess the competitiveness of the British economy in 1850, we try to establish an ideal type of 'perfect competition' we shall at once find that we can only define it in terms of the preferences, information and relationships of individuals—an assertion which can be confirmed by turning to any economics text-book. In other words, we shall have established an *individualistic* ideal type.[1] But if knowledge of the general characteristics of a social situation is always derivative knowledge, pieced together from what is known of individuals' situations, then it is not possible for historical analysis to proceed *from* overall characteristics *towards* individuals' situations. The former is logically derivative from the latter. Weber's earlier conception of an ideal type presupposed that one can detect the essential traits of some historic 'whole' while remaining aloof from the detail of personal behaviour; but this belief is shown to be false when we actually construct such a type. It was probably this experience which later led Weber tacitly to abandon holistic ideal types and the impossible method associated with them, in favour of individualistic ideal types and the method of reconstructing historical phenomena with their aid.[2]

[1] Similarly, if we try to construct an ideal type for 'feudalism,' say, we shall at once find ourselves speaking of people's obligations and privileges towards their superiors, inferiors, the land, and so on.

[2] What I call a 'holistic ideal type' roughly corresponds to what Eucken called a 'real type,' a name he used to denote the 'stages,' such as 'city economy,' 'early capitalism,' 'mature capitalism,' through which, according to the Historical School of economists, any economic system develops. He also rejected such types in favour of individualistic ideal types (which he simply called 'ideal types') and he criticised Weber for confusing the two, but from a somewhat different viewpoint to my own. His fascinating book, *The Foundations of Economics*, contains a sustained plea for the

The assertion that knowledge of social phenomena can only be derived from knowledge about individuals requires one qualification. For there are certain overt features [1] which can be established without knowledge of psychological facts, such as the level of prices, or the death-rate (but *not* the suicide-rate). And if we detect more or less regular changes in such overt features we have something eminently suitable for analysis. But some people, over-impressed by the quasi-regularity of, for example, a long-term ' wave ' in economic life, have supposed that such a thing possesses a sort of internal dynamic, and obeys its own laws ; and that while *it* must therefore be taken as a datum, many other phenomena (such as bursts of inventiveness, emigration movements, outbreaks of war) can be explained as consequences of it.[2]

The Israelites also imputed their fortunes and misfortunes to a superior entity immune from their own activities ; but they rightly called this ' God.' All social phenomena are, directly or indirectly, *human* creations. A lump of matter may exist which no one has perceived, but not a price which no one has charged, or a disciplinary code to which no one refers, or a tool which no one would dream of using. From this truism I infer the methodological principle which underlies this paper, namely, that the social scientist can continue

fertile marriage of abstract theory and concrete fact, and a powerful criticism of the Historical School for blurring the distinction between the two ; whereas I am arguing against methodological holism, and for methodological individualism. Our arguments tend to coincide because ' historicism ' is closely related to ' holism ' : the belief in laws of development presupposes a ' whole ' which undergoes the development. For his discussion of real and ideal types, see pp. 69–70, 173, 300, 326, 331, and especially pp. 347–9.

[1] By ' overt feature ' I do not mean something which can necessarily be directly perceived—it may be a highly theoretical construct. But whether it be the price of a marked article in a shop-window, or the average level of prices in 1815, an overt feature is something which can be ascertained without referring to people's dispositions, etc. See R. Stone, *The Role of Measurement in Economics*, Cambridge, 1951, p. 9.

[2] I have written the above with the Russian economist Kondratieff in mind. He asserts that the view that long waves ' are conditioned by casual, extra-economic circumstances and events, such as (1) changes in technique, (2) wars and revolutions, (3) the assimilation of new countries into the world economy, and (4) fluctuations in gold production . . . reverse[s] the causal connections and take[s] the consequence to be the cause.' (N. D. Kondratieff, ' The Long Waves in Economic Life,' *Readings in Business Cycle Theory*, Blakiston Series, London, 1950, ch. 2, p. 35.) In other words, the long wave is *the* fundamental datum, in terms of which all political, technological, etc., changes must be explained.

searching for explanations of a social phenomenon until he has reduced it to psychological terms.[1] I am not, of course, denying that such a thing as a long-term price movement will partially determine other events, which will be partially explicable in terms of it. I only assert that it too is, in principle, explicable, and explicable in terms of individual attitudes towards things and other people.

To sum up my argument so far : An understanding of a complex social situation is always derived from a knowledge of the dispositions, beliefs, and relationships of individuals. Its overt characteristics may be *established* empirically, but they are only *explained* by being shown to be the resultants of individual activities.[2]

All this was recognised by the later Weber. In *The Theory of Social and Economic Organisation* ideal type construction means (not detecting and abstracting the overall characteristics of a whole situation, and organising these into a coherent scheme, but) placing hypothetical, rational actors in some simplified situation, and in deducing the consequences of their interaction.

Such intellectual experimenting *may* be fruitful even if some of the premisses are very unrealistic. For instance, the concept of a static economy in equilibrium aids the analysis of the changes and disequilibria of actual economies. And gross exaggeration of one factor may show up an influence which would otherwise have been overlooked. This is particularly important in social science where the influence of different factors can seldom be accurately calculated. If

[1] Professor M. Ginsberg takes a contrary view : 'These principles [of historical interpretation] are not necessarily exclusively psychological or even teleological : there may well be social laws *sui generis* . . .' (*Aristotelian Society, Supplementary Volume*, XXI, 1947, 'Symposium : The Character of a Historical Explanation,' p. 77). The only example he gives of something determined by such laws is phonetic change. But I think that the problem of phonetic change is so different from the typical problems of the social sciences (war, unemployment, political instability, etc.) that there is a better case for leaving it outside their domain than there is for widening their scope and methods to let it in. The study of this kind of problem might be classed with vital statistics and the study of contagious diseases which, Professor Hayek has argued, should be regarded as 'natural sciences of society' rather than as 'social sciences.' (*Individualism and Economic Order*, London, 1949, p. 57.)

[2] An explanation may be in terms of the *typical* dispositions of more or less anonymous individuals, or in terms of the dispositions of specific individuals. (This is the basis of my distinction between 'explanation in principle' and 'explanation in detail.' See p. 30.) Thus, you might try to explain an election result in terms of how 'the Lancashire shop-keeper' and 'the non-party professional man' etc., felt ; or, if you had an unlikely amount of knowledge, in terms of the dispositions of each elector.

E is the *sort* of effect produced by F_1, and if F_1 and E are both present, the social scientist tends to assume that F_1 is *the* cause of E, whereas F_1 may have caused only a *part* of E, and an undetected factor F_2 may have caused the rest of E. For example, the domestic economic policy of country A will be a major influence on its own economy ; but this may also be influenced by the domestic economic policy of country B. In order to show up this secondary influence, we might assume provisionally that A exports *all* its production to, and imports *all* its consumption from, B, and then deduce the effect on A of a change of policy in B.[1]

But that would be a preliminary intellectual experiment. The premisses of a finished ideal type should be sufficiently realistic for it to be applicable to historical situations. I now turn to the problem of application.

3 *Historical Explanation*

I shall consider three levels of historical explanation : (I) colligation (where ideal types play no significant role) ; (II) explanation in principle (which is the field *par excellence* for ideal types) ; and (III) explanation in detail (where ideal types are mostly constructed *ad hoc*, and rendered increasingly realistic until they become empirical reconstructions).[2]

(I) *Colligation.* The term ' colligation ' has been revived by Mr Walsh[3] to denote a procedure which is important, not because it is methodologically powerful, but because most ' literary ' historians do in fact use it when they write, for example, constitutional history. It means ' explaining an event by tracing its intrinsic relations to other events and locating it in its historical context.'[4] Thus we begin to understand why a bill was enacted in May 1640 condemning Strafford to death when we learn of such matters as : his autocratic power in Ireland ; Parliament's fear of the Irish army and Pym's ruthlessness as

[1] I owe this example to Professor J. E. Meade.

[2] I understand that Professor F. A. Hayek also draws a distinction between explaining in principle and explaining in detail, but that he wishes to distinguish an explanation of why, say, a price rose, from a quantitative explanation of the amount by which it rose ; whereas I wish to distinguish between explanations in terms of *typical* dispositions, etc., and explanations in terms of the characteristics and personal idiosyncrasies of the principal actors concerned.

[3] See W. H. Walsh, *An Introduction to Philosophy of History*, London, 1951, ch. 3, § 3

[4] Op. cit. p. 59

a parliamentary leader ; the King's dependence on Parliament to pay indemnities to the Scottish army in the north ; and the angry anti-royalist mob which beset Westminster during the bill's passage. It may also be better understood by being colligated with *subsequent* events. Thus the Long Parliament's later treatment of Laud and Charles suggests that its treatment of Strafford was not eccentric, but part of a campaign against extra-parliamentary power.

However, as Mr Walsh admits, colligation yields only what he calls a ' significant narrative ', which is more than a chronicle, but less than a full explanation, of the events colligated.

(II) *Explanation in Principle.* The principle of the automatic governor can be demonstrated in a simple model which shows that a fall in some temperature, voltage, speed, pressure, etc., below a certain level will move a lever which will increase the supply of heat, etc. ; and *vice versa.* Understanding this, you can explain the constant temperature of your car's circulating water *in principle* if you know that an automatic governor controls it, although you do not understand its detailed operation.[1]

Analogous explanations are used in applied economics. Consider the bargaining process. The principle of this is demonstrated in the following ideal type. Two rational agents are postulated. Each possesses one homogeneous, divisible good, and each knows the schedule of those combinations of various portions of his own and the other's good which he would exchange indifferently for the whole of his present good. These premisses are highly precise, and also highly formal. Call them α and β. From these it is deduced that only the limits within which a bargain will be struck are determined, and that within those limits the outcome will be arbitrary. Call this consequence ω. Now consider post-war Anglo-Argentinian trade negotiations. Here, we can, I think, detect factors A, B, c, d, . . . Z where : A and B are the resources and policies of the trade delegations, and are concrete examples of α and β ; c, d, . . . are minor factors whose small influences on Z may partly cancel out ; and Z is the outcome of the negotiations, the actual instability of Anglo-Argentinian trade relations, which is a rough empirical counterpart of ω. The ' α, β, ω ' ideal type explains in principle the ' A, B, c, d, . . . Z ' situation.[2]

In this example I have assumed that only one main economic

[1] It is the principle of an invention rather than its physical detail which is usually described in patents. See M. Polanyi, *The Logic of Liberty*, London, 1951, p. 21.

[2] I owe this example to Professor Lionel Robbins.

principle, demonstrable in a single ideal type, was at work in the historical situation. But the situation will usually be more complex. Consider a wage-bargain. Perhaps there is a closed shop and limited entry into the trade union. The firm, a centrally planned organisation, buys its raw materials, which are rationed, through a government agency, and its machinery at the best price it can get from oligopolistic suppliers. By law it must export a proportion of its produce, and the export market is highly competitive. Its home prices are fixed by a cartel agreement.[1] The general situation is inflationary.

Here, the outcome of the bargaining process will be shaped by a number of economic principles besides that illustrated in the previous example. And in order to understand the whole situation in principle it would be necessary to build up a complex model from the relevant simple ideal types. Here, an ' (α,β), (λ,μ), (σ,τ), . . . ω ' model would be used to explain in principle an ' A, B, c, . . . L, M, n, . . . S, T, u, . . . Z ' situation (where c . . ., n . . ., u represent comparatively uninfluential factors).

The social scientist's explanations in principle lack the quantitative precision of explanations in mathematical physics. But he may claim that his explanations are at any rate ' intelligible ' and ' satisfying,' whereas those of the natural scientist are not. The most universal laws which the latter applies in his explanations and predictions contain terms (e.g. ' elementary quantum of action ') whose connotation the layman cannot ' picture ' or ' grasp intuitively.' Moreover, the status of these most universal laws is probably only temporary : they will probably come to be subsumed under higher order laws.

But the ultimate premisses of social science are human dispositions, i.e. something familiar and understandable. They ' are so much the stuff of our everyday experience that they have only to be stated to be recognised as obvious.'[2] And while psychology may try to explain these dispositions, they do provide social science with a natural stop-

[1] The definitions of perfect competition, oligopoly and monopoly provide, incidentally, good illustrations of the principle of methodological individualism. An entrepreneur faces : (*a*) perfect competition if the price at which he sells is determined for him ; (*b*) oligopoly, if he can alter his price, but if this alteration may lead to price changes by his competitors which may force him to make further, undesired alterations to his own price ; and (*c*) monopoly, if he can alter his price without causing undesired repercussions. Competition, oligopoly and monopoly are nothing but the outcome of the behaviour of interacting individuals in certain relationships.

[2] Lionel Robbins, *The Nature and Significance of Economic Science*, London, 1935, p. 79

ping-place in the search for explanations of overt social phenomena. The social scientist might claim more. The natural scientist cannot, strictly speaking, *verify* valid hypotheses ; he can only *refute* false ones.[1] He can say, ' If H, then E. But not-E. Therefore not-H.' But if he says, ' If H, then E. Moreover E. Therefore H ' he commits the fallacy of affirming the consequent.[2] But a social scientist might claim that a valid social theory *can* be verified because both its conclusions *and* its premisses can be confirmed—' you assent to the former because they correspond with recognised social facts ; and you assent to the latter because they correspond with your ideas of how people behave.' An example of the belief that a social theory can be wholly verified by being confirmed at both ends is to be found in Keynes' *General Theory*. There he asserts ' the fundamental psychological law, upon which we are entitled to depend with great confidence . . . from our knowledge of human nature . . . , that men are disposed, as a rule and on the average, to increase their consumption as their income increases, but not by as much as the increase in their income ' ; and *vice versa*.[3] He then shows that the empirical fact that no depression has worsened until ' no one at all was employed ' is a deductive consequence of this law.[4] The theory is thus doubly confirmed, and therefore verified : ' it is *certain* that experience would be extremely different from what it is if the law did not hold.' [5] No natural scientist could claim so much for *his* laws. His explanations are ' surprising ' in the sense that he explains the familiar in terms of the unconfirmable unfamiliar. So are the explanations of a Freudian psychologist. But the social scientist explains the familiar in terms of the familiar. The element of surprise in *his* explanations lies in the logical demonstration of connections which had not been seen before between facts which are *prima facie* discrete.

But a double caution must be entered against the idea of double confirmation in social science : (i) The same conclusion can, of course, be deduced from different sets of premisses, and we cannot be certain that our set of psychological assumptions is the correct set. (ii) Even if our psychological assumptions *are* correct, and even if we *do* find that

[1] See K. R. Popper, *Logik der Forschung*, Vienna, 1935, *passim*

[2] See F. S. C. Northrop, *The Logic of the Sciences and the Humanities*, New York, 1948, pp. 108–9 ; and e.g. H. W. B. Joseph, *An Introduction to Logic*, Oxford, 1916, pp. 522–3

[3] *The General Theory of Employment, Interest and Money*, London, 1936, p. 96

[4] Op. cit. p. 252

[5] Op. cit. p. 251 (my italics)

their deductive consequences correspond to recognised facts, we may nevertheless be mistaken if we explain these facts as a consequence of those psychological factors. This is because we can seldom calculate the relative influence of different psychological factors (see p. 29).[1] Thus Keynes' belief that people are disposed to save a smaller proportion of their income if their income diminishes may well be correct; his demonstration that this general disposition would not allow depressions to worsen indefinitely is immaculate; and the fact that depressions do not worsen indefinitely is undoubted. It is nevertheless conceivable that *no* depression has been halted because of this disposition. One may have been halted by an outbreak of war, another by an upsurge of confidence, another by a public works policy, and so on. In explaining social phenomena we must not be content with the detection of one factor which, singly, would have produced, after an unstated period, an unstated amount of an effect which may, in any particular situation, have been caused mainly by quite different factors.

If I am right in supposing that social theories derive sociological conclusions from psychological premisses, we should expect to find that major theoretical advances in social science consist in the perception of some typical feature of our mental make-up which had previously been disregarded, and in its formulation in a way which is more deductively fertile and which goes to explain a wider range of facts, than the psychological generalisations relied on hitherto. And this is precisely what we do find. I think that it would be generally conceded that economics is the most mature social science, and that the two most striking advances made in economics during the last century are: (i) the 'revolution' which occurred in the early 1870's when Jevons, Menger and Walras introduced the concept of marginal utility; and (ii) the Keynesian 'revolution.'

(i) The classical economists saw that the price of a good must be partly determined by the demand for it, and that that demand must reflect the buyers' estimates of the good's utility—and yet diamonds, whose utility is low, fetch a far higher price than water, whose utility is high. So they tried to escape from their dilemma by saying that the price of a good is determined by the cost of its production, though this would obviously be untrue of an unwanted good which had been expensively produced. This difficulty dissolved with the introduction of the idea of the utility, not of a whole good, but of its least important,

[1] I was myself inclined to accept the idea of double confirmation until Professor Popper pointed out to me the relevance of this consideration.

or ' marginal,' unit. For—and this is the recognition of a psychological contour-line which had not been clearly mapped before—it is in terms of that unit that we tend to value a whole good ; and the more we have of the same good, the more its marginal utility diminishes. Hence, if diamonds became abundant and water very scarce, their subjectively determined values would be reversed. F. H. Knight has given a vivid description of the elegance and power of the concept of marginal utility :

> To its admirers it comes near to being the fulfilment of the eighteenth-century craving for a principle which would do for human conduct and society what Newton's mechanics had done for the solar system. It introduces simplicity and order, even to the extent of making it possible to state the problems in the form of mathematical functions dealt with by the methods of infinitesimal calculus.[1]

(ii) The reader who is unfamiliar with Keynes' contribution to the theory of employment must take its value on trust, for it is impossible to describe it briefly. But here again we find that what it rests on is the perception and precise formulation of certain human dispositions which Keynes regarded as ' ultimate independent variables,' [2] and from which he could deduce such dependent variables (or overt phenomena, as I have called them previously) as the amount of employment and the general level of prices. At the heart of his *General Theory* Keynes placed ' three fundamental psychological factors, namely, the psychological propensity to consume, the psychological attitude to liquidity and the psychological expectation of future yield from capital-assets.' [3]

(III) *Explanation in Detail.* The mark of an explanation in principle is its reliance on typical dispositions and its disregard of personal differences. But it is often impossible to disregard these, for instance, in diplomatic history. Here, the premisses of a historical explanation must be the specific dispositions, beliefs and relationships of actual people. This is what I call ' explanation in detail.'

So far, I have allowed two questions to lie dormant : (i) What is the status of these dispositions, and wherein lies their explanatory power ? (ii) What assumptions concerning people's rationality are we obliged to make when we explain something in terms of their dispositions and beliefs ? These questions were not acute so long as

[1] F. H. Knight, *The Ethics of Competition*, London, 1935, p. 158

[2] Op. cit. p. 246. Of course, the variables are only ' independent ' from the social scientist's point of view. The psychologist would probably consider them ' dependent '.

[3] Op. cit. pp. 246–7

explanations in principle were being considered. An explanation requires a general statement as its major premiss; and when we postulate a typical disposition we assert that all men (with trivial exceptions and minor deviations, and, perhaps, within a limited historico-geographical area) are prone to behave in a certain kind of way; and this gives us the generality we require. And we explain in principle by combining types which are, after all, ideal, and which may therefore be expected to contain idealised simplifications of real life, such as the assumption of fully rational behaviour in the light of preferences and beliefs.

But when we turn to explanations in detail these two questions do become acute. For we are here concerned with the variegated dispositions of actual people, and these appear to lack the generality which the major premiss of an explanation needs. And actual people do not behave altogether rationally, which suggests that we cannot go on assuming that they do. I shall discuss the first question under the head of 'Personality', and the second under the head of 'Rationality and Purposefulness'.

(i) *Personality*. A series of occurrences constitutes a person's life, and a complex and evolving system of dispositions constitutes his personality.[1] Dispositions 'are not laws, for they mention particular things or persons. On the other hand they resemble laws in being partly "variable" or "open."'[2] The dispositions which comprise a unique personality are, so to speak, 'laws' which apply to only one man over a limited period of time. It is as if the laws of chemistry concerning, say, mercury, applied only to a period in the life of one

[1] I have adopted the terminology of Professor G. Ryle's *Concept of Mind* (London, 1949; see especially ch. 5), but not that book's famous denial that a man has 'privileged access' to his own mind. Sitting beside the driver of a car who turns white and wrenches the steering-wheel over, I may perceive instantaneously *that* he fears an accident, but I do not *feel* his fear. Moreover, the historian is usually in the position of the policeman who tries to reconstruct what happened from skid-marks and reports of witnesses; and for him the dualism between uninterpreted overt behaviour (e.g. Jan Masaryk's fall from a Prague window) and its interpretation in psychological terms is very real.

But the following characteristic remarks suggest that Professor Ryle has now modified his original anti-dualism: 'We have . . . a sort of (graduatedly) privileged access to such things as palpitations of the heart, cramps, and creaks in the joints.' 'I have elsewhere argued for the idea that a tickle just *is* a thwarted impulse to scratch. . . . But I do not think now that this will do.' ('Feelings,' *The Philosophical Quarterly*, April 1951, 1, 198–9)

[2] Ryle, op. cit. p. 123

solitary bottle of mercury which has come into existence, matured, and will dissolve, and whose twin, we may confidently assume, never has existed, and never will.

All this presupposes that men do have personalities, i.e. that their behaviour is fairly consistent over a period of time if their personalities are not subjected to dissolvent shocks. This assumption of the quasi-permanence of personalities corresponds roughly—very roughly—to the natural scientist's belief in the permanence of the natural order.

The generalisations of psychology fit into this scheme in the following ways : (*a*) Some attribute a certain disposition to all men. The theory of the association of ideas is an example. (*b*) Others attribute certain dispositions to a certain type of man, e.g. the 'introvert.' (*c*) Yet others attempt to describe the dynamics of personality-development, deriving later dispositions from prior determining conditions in the light of psychological theory. (It is this search for the primitive determining conditions which leads back to the 'formative years' of early childhood.) An example is the theory of the 'incest-complex,' which asserts that a man who idealised his sister as a child will be prone to hypoæsthesia on marriage.

A disposition attributed to one man is no weaker than the same disposition attributed to all men in explaining and predicting that one man's behaviour. 'X will accept office' can be deduced from the minor premiss, 'X believes that if he refuses the office he has been offered he will find himself in the wilderness' in conjunction with *either* (*a*) the major premiss, 'All men seek power,' *or* (*b*) the major premiss, 'X is a power-seeker'; but whereas (*b*) may be true, (*a*) is the sort of statement which is likely to be false because men are not uniform.[1]

Similarly, a detailed description of one man's chess-playing dispositions (his knowledge of the rules, evaluations of the different pieces, and ability to see a certain number of moves ahead) together with his present beliefs about his opponent's intentions and the positions of the pieces, imply his next move, which could not be deduced from propositions about chess-players in general in conjunction with a description of the present state of the game.

Thus the idea that the historian's interpretative principles are simply generalisations about human nature, into which he must have

[1] On law-like dispositions of very limited generality, see R. Peters, 'Cure, Cause and Motive,' *Analysis*, April 1950, **10**, no. 5, 106

special insight, is inadequate.[1] His knowledge of human nature in general has to be supplemented by a knowledge of the peculiar personalities of the principal actors concerned in the situation he is trying to understand, whether his problem be X's behaviour, or the chess-player's next move, or the rise of Christianity, or the Congress of Vienna.

The dispositions which the historian attributes to a personality he is trying to reconstruct resemble scientific laws in two further ways.

(*a*) They are postulated hypotheses which correspond to nothing observable, although observable behaviour can be inferred from them in conjunction with factual minor premisses. Consequently, in judging their validity we want to know, not the mental process by which the historian arrived at them, but their degree of success in accounting for what is known of the man's behaviour. The hypothetical dispositions postulated by the historian who has 'sympathetically identified himself with his hero' may be richer than those of the historian who has not done so, but it is not this which gives them a certificate of reliability. Professor Hempel has put the matter very clearly :

> The method of empathy is, no doubt, frequently applied by laymen and by experts in history. But it does not in itself constitute an explanation ; it is rather essentially a heuristic device ; its function is to suggest certain psychological hypotheses which might serve as explanatory principles in the case under consideration.[2]

And the historian is no more precluded from reconstructing a strange and unsympathetic personality than is the scientist from reconstructing the behaviour of an atom which does things he would not dream of doing himself.[3]

[1] This idea underlies Mr Walsh's contribution to the symposium on 'The Character of a Historical Explanation' (*Aristotelian Society, Supplementary Volume* XXI, 1947.) From it he infers that, since 'men's notions of human nature change from age to age' we must recognise 'the subjective element which history undoubtedly contains' (p. 66). The point is, do historians' notions of, say, Napoleon's personality change from age to age (not because of the discovery of fresh evidence, etc., but) arbitrarily ?

[2] Op. cit. p. 467. Failure to realise this is, I think, the weakness of R. G. Collingwood's *The Idea of History* (ed. T. M. Knox, Oxford, 1946).

[3] Failure to recognise this vitiates, I think, some of the argument in Professor F. A. Hayek's 'Scientism and the Study of Society' (*Economica*, 1942, **9**, 267-291 ; 1943, **10**, 34-63 ; 1944, **11**, 27-39). There, despite all the work done in abnormal psychology, he asserts : 'When we speak of mind what we mean is that certain

(*b*) The dispositions which constitute a personality also resemble scientific laws in that they form a hierarchical system ; and this is of considerable methodological importance. It is, of course, essential that the dispositions which a historian attributes to a historical figure should not be mere *ad hoc* translations of known occurrences into dispositional terms. It is no explanation of Brutus' behaviour to say that he was disposed to assassinate Caesar, though it would be a ground for an explanation to say that Brutus was disposed to place his loyalty to the State above his loyalties to his friends, if independent evidence were found to support this hypothesis. Moreover—and it is here that the idea of a hierarchy of dispositions is important—the historian who can explain some aspect of a person's behaviour *up to a certain time* in terms of certain disposition, although his *subsequent* behaviour conflicts with this disposition, must not merely say that at that time the earlier disposition gave way to another. He should find a *higher order* disposition which helps to explain both earlier and later lower order dispositions, and hence the whole range of the person's behaviour. For example : suppose that Russian foreign policy is controlled by a consistent, integrated personality. Before 1939 Russia was disposed to pursue an anti-fascist foreign policy. But in 1939 came the Russo-German Pact. In order to explain this aberration it is not enough for the historian to say that the anti-fascist disposition was replaced. He must find a higher order disposition (e.g. ' Russian foreign policy is determined by considerations of national expediency, not by ideological factors ') from which, in conjunction with factual premisses, the change in policy is derivable. In doing this it is clear that the historian will *not* be translating an occurrence (the signing of the pact) into dispositional terms, but deriving both the occurrence and the change in lower order dispositions from a more permanent and fundamental disposition.

In conclusion it should be said that the personality of a man in society comprises dispositions both of a more private and temperamental kind, and of a more public and institutional kind. Only

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phenomena can be successfully interpreted on the analogy of our own mind. . . . To recognise mind cannot mean anything but to recognise something as operating in the same way as our own thinking.' From this false premiss he correctly infers the false conclusion that ' history can never carry us beyond the stage where we can understand the working of the minds of the acting people because they are similar to our own ' (vol. **10**, pp. 61-2). Only a war-like historian can tackle a Genghiz Khan or a Hitler !

I hasten to add that I owe much to other parts of Professor Hayek's argument, and that I suspect that my paper contains a good deal of unwitting plagiarism of it.

certain individuals are disposed to weep during the death-scene in *Othello*, but all policemen are disposed to blow their whistles under certain circumstances and any Speaker in the House of Commons is disposed to disallow parliamentary criticism of exercises of the Prerogative. And these more public and institutional dispositions, which may vary very little when one man undertakes another's role, can be abstracted from the total, variegated flux of dispositions, and so provide the social scientist with a fairly stable subject-matter.[1]

(ii) *Rationality and Purposefulness.* Before asking what assumptions the historian is obliged to make about the rationality of those whose behaviour he is trying to interpret, we must establish a satisfactory 'definition in use' of the term 'rational behaviour.' Weber defined it, very austerely, as the deliberate and logical choice of means to attain explicit goals, in the light of existing factual knowledge. This is unsatisfactory for two reasons. (*a*) Whitehead said somewhere that 'civilisation advances by extending the number of important operations we can perform without thinking about them.' This morning's tooth-brushing was not irrational because done from habit and not from deliberations on dental hygiene. Our pursuit of goals need not be conscious in order to be rational. (*b*) Behaviour often does not conform to the end-means pattern. I may tell the truth, or go fishing, simply from a desire to do so, with no further end in mind.[2]

We escape these difficulties by saying that a person has behaved rationally if he *would* have behaved in the same way if, with the same *factual* information, he had seen the full *logical* implications of his behaviour, whether he actually saw them or not. And if we define purposeful behaviour as trying (consciously or otherwise) to do or achieve something wanted, it follows that fully rational behaviour is a limiting case of purposeful behaviour.

The historian who tries to interpret overt behaviour must assume that it is purposeful but not necessarily fully rational.[3] Consider a *crime passionel* committed by an enraged husband. A judge who assumed that the husband had behaved purposelessly could reconstruct the event in a number of quite arbitrary ways—perhaps cramp caused

[1] See Hayek, op. cit. vol. 9, p. 284.

[2] 'We invest our capital reluctantly in the hope of getting dividends. . . . But the angler would not accept or understand an offer of the pleasures without the activities of angling. It is angling that he enjoys, not something that angling engenders.' Ryle, op. cit. p. 132. See also H. A. Pritchard, *Moral Obligation*, Oxford, 1949, pp. 10–11.

[3] See Robbins, op. cit. ch. 4, sect. 5

his finger to contract round the trigger of a gun which happened to be pointing at his wife's lover.[1] But while the judge must not assume purposelessness he need not assume full rationality. The husband would probably have confined himself to threats and remonstrances if he had paused to consider the less immediate consequences of a violent course of action.

The assumption of purposefulness is constantly made by those who attempt the most intensive analysis of human behaviour, i.e. practising psycho-analysts. It has often been pointed out that the psycho-analyst is on the side of rationality in that he tries to cure his patients. More interesting from our point of view is his assumption that the behaviour of an *uncured* patient is thoroughly purposeful. Suppose a patient forgets to wind his watch, and so arrives late at his father's funeral. Unlike the layman, the psycho-analyst will not attribute the stopped watch to accidental forgetfulness, to a purposeless psychic aberration. He will ask his patient *why* he *wanted* his watch to stop—maybe he felt guilty on having a death-wish fulfilled and so created an excuse for avoiding the funeral. This would certainly be purposeful behaviour, and might even be regarded as rational behaviour based on misinformation.[2]

(iii) *Conclusion.* Having considered the status of dispositions and the problem of rationality, we can now return to explanations in detail.

Weber advocated using individualistic ideal types, which depict rational behaviour, to show up the partial irrationality of actual behaviour. But this is unacceptable. Suppose that a historian wishes to interpret a general's behaviour during a battle. He has reconstructed, as best he can, both the dispositions which constitute that aspect of the general's personality with which he is concerned, and the general's information about the military situation. Suppose that, in conjunction, these dictate retreat as the rational course of action, but that the general is known to have given the signal to advance. Now the historian, like the psycho-analyst, will not want to leave puzzling overt behaviour uninterpreted; but according to Weber he should simply call this a deviation from the ideally rational course of action implied by the premisses of his theoretical reconstruction of the situation. But since an irrational aberration can be attributed to

[1] As Professor Popper once said to me, 'Judges have a vested interest in murder!'

[2] The mixture of rationality and misinformation due to childhood associations which psycho-analysis brings to the surface was pointed out to me by Professor Popper.

anything from boredom to panic, this procedure would result in thoroughly arbitrary reconstructions. Rather, the historian must discover the most satisfactory amendment to the premisses of his ideal type (constructed more or less *ad hoc* to depict the main features of the general's personality and situation) which will remove the discrepancy between what it implies and what happened.[1] Perhaps there is independent evidence to suggest that the general was more lion-hearted than the historian had supposed ; or perhaps he had under-estimated the enemy's strength, or, in estimating the immediate consequences of an advance, he had overlooked a more distant un-desirable repercussion. When *ad hoc* ideal types are used in detailed historical explanations, they have to be amended and amended until they cease being ideal constructs and become empirical reconstructions. The historian who claims to have interpreted a historical situation should be able to show : (*a*) that the behaviour of the actors in it flows from their personalities and situational beliefs ; and (*b*) that significant events which no one intended are resultants of the behaviour of interacting individuals.

4 *Summary*

An individual's personality is a system of unobservable dispositions which, together with his factual beliefs, determines his observable behaviour. Society is a system of unobservable relationships between individuals whose interaction produces certain measurable sociological phenomena. These, together with observable individual behaviour, are the only overt facets of a social system. We can apprehend an unobservable social system only by reconstructing it theoretically from what is known of individual dispositions, beliefs and relationships.[2] Hence holistic ideal types, which would abstract essential traits from a social whole[3] while ignoring individuals, are impossible : they

[1] On satisfactory and unsatisfactory amendments of systems of dispositions, see p. 39.

[2] 'The social sciences . . . do not deal with "given" wholes but their task is to *constitute* these wholes by constructing models from the familiar elements . . .' 'The whole is never directly perceived but always reconstructed by an effort of our imagination. Hayek, op. cit. vol. **10**, p. 44, and p. 42 n. 1.

[3] Weber certainly did not equate 'essential traits' with overt, measurable phenomena. Indeed, I do not think he would have regarded the latter even as candidates for inclusion among the former. By an 'essential trait' he meant what he called a *meaningful* concept, or historical cluster of ideas, such as 'Christianity,' 'methodism,' 'socialism' (see *Methodology*, pp. 94-7). But an uninterpreted, unexplained, observable sociological phenomenon, such as a price-level, is not meaningful in this sense.

always turn into individualistic ideal types. Individualistic ideal types of explanatory power are constructed by first discerning the form of typical, socially significant, dispositions, and then by demonstrating how, in various typical situations, these lead to certain principles of social behaviour.

If such a principle, or a number of such principles, is at work in a historical situation, the outcome of that situation can be explained anonymously, or in principle, by an application to it of the relevant ideal type, or combination of ideal types. If the idiosyncrasies of the actors concerned significantly influenced the outcome, it must be explained in terms of their peculiar dispositions and beliefs. In either case, the hypothetico-deductive method is used. The hypotheses consist of postulated dispositions, beliefs and relationships of (anonymous or specific) individuals ; and their test lies in the correspondence or otherwise between their deductive consequences and what is known of the overt characteristics of the situation being reconstructed. How the historian establishes the overt characteristics of a vanished situation is another story.

The London School of Economics and Political Science
Houghton Street, Aldwych, London W C 2

CAN A MECHANICAL CHESS-PLAYER OUTPLAY ITS DESIGNER ? *

W. Ross Ashby

1 *Introduction*

Mr Adkins' letter [1] is welcome, for it insists on the discussion of a question often heard in conversation but not yet, so far as I am aware, adequately treated in print : to what extent is the machine restricted by the limitations of its designer ? The best form of the question is uncertain, but we can usually recognise it in its different forms. Thus, Descartes declared that there must be at least as much reality and perfection in the cause as in the effect. Kant [2] asked, ' How can work full of design build itself up without a design and without a builder ? ' Another form has been used as title for this paper.

The question is not only of philosophic interest but is fast approaching practical importance. Some of us are studying the physiology of the brain by building models that will mimic its characteristic properties. Our working hypothesis is that we can eventually build a ' real ' brain. But for it to be a ' real ' brain it must produce cleverness of its own and not just give us back the ingenuity we have put into it. Here the truth or falsity of Descartes' dictum is crucial. If it is true, we are wasting our time ; only if it is false can we succeed. So we must try to disprove it. If we can prove it false, not only may the way be shown open to a ' real ' brain but, by scrutinising the proof, we may be able to get a hint about how to proceed, for the proof may show what the real difficulty is, and how it can be overcome.

2 *Measuring ' Design '*

Examining Descartes' dictum, we notice first that it is essentially quantitative. It says, in effect : if the quantity of design supplied by the designer is D, and if the quantity of design shown by the machine is M, then necessarily

$$D \geqslant M \quad . \quad . \quad . \quad . \quad . \quad (1)$$

* Received 11. xii. 51

[1] This *Journal*, 1951, **2**, 248

[2] *General History of Nature*, 1755

Descartes, however, gave no hint as to how these quantities were to be measured, so we must develop a method. Fortunately we need not enquire exhaustively into all that is implied by 'design,' for the measurement of a quantity can properly precede the understanding of what it is that is being measured—engineers were measuring the 'electric fluid' and were lighting towns with it several decades before its real nature was understood.

How are we to obtain an objective and consistent measure of the 'amount of design' put into, or shown by, a machine? Abstractly, 'designing' a machine means giving selected numerical values to the available parameters. How long shall the lever be? where shall its fulcrum be placed? how many teeth shall the cog have? what value shall be given to the electrical resistance? what composition shall the alloy have? and so on. Clearly, the amount of design must be related in some way to the number of decisions made and also to the fineness of the discrimination made in the selection. I suggest that the measure appropriate to our purpose is one already developed in Shannon's theory of information.[1] Though this theory was developed by communication engineers for technical purposes, it will, I believe, be found to have applications, and to be of philosophic importance, over a much wider range. Its use enables us to treat the question quantitatively, and it provides the discussion with a secure basis of experimental and practical experience.

Shannon's measure is defined fundamentally as follows. Suppose certain events $E_1, E_2, \ldots, E_n$ have probabilities $p_1, p_2, \ldots, p_n$, respectively, of occurring, with

$$p_1 + p_2, + \ldots + p_n = 1,$$

then the occurrence of the actual event is associated with the quantity

$$-\Sigma_j p_j \log p_j \qquad \ldots \qquad (2)$$

(If the probabilities are all equal, and each equal to $1/n$, then the quantity becomes simply $\log n$.) As an example, suppose that we are going to draw and inspect one card from a shuffled pack and that we are interested only in the distinction between the three events

E_1 : the drawing of the King of Clubs,
E_2 : the drawing of any Spade,
E_3 : the drawing of any other card;

[1] C. E. Shannon, 'A mathematical theory of communication,' *Bell System tech. J.*, 1948, **27**, 379-423, 623-656; Norbert Wiener, *Extrapolation, Interpolation and Smoothing of Stationary Time Series*, New York, 1949

then $p_1 = \frac{1}{52}$, $p_2 = \frac{1}{4}$, $p_3 = \frac{19}{26}$; and the quantity associated with the drawing of the card, and the discovery of which event has actually occurred, is

$$-\tfrac{1}{52}\log\tfrac{1}{52} - \tfrac{1}{4}\log\tfrac{1}{4} - \tfrac{19}{26}\log\tfrac{19}{26},$$

which, if the logarithms are to the base 2 (see below), has the value 0·94. This number measures the amount of information, relative to these E's, that is to be obtained by drawing a card.

The number 0·94, it should be noticed, does not belong to any particular card, for it can be calculated before the drawing takes place. It does not answer the question 'I have drawn the Three of Hearts: how much information have I received?'; rather it answers the question 'when I draw and look at the card, how much uncertainty will be removed?' It measures, primarily, the uncertainty existing before the drawing is made. This uncertainty will be dispelled by the actual drawing and inspection of the card, and may be used as a measure of the information to be expected in the drawn card if we regard 'information' as 'that which dispels uncertainty.' At first impression this way of measuring information may seem unnatural, almost the reverse of what one would expect; but its usefulness in communication engineering has put its practical value and the soundness of its method beyond doubt.

The amount can be measured in various units, which depend on the base used for the logarithms. The most convenient here is the 'bit'[1]—a contraction of BInary digiT—which is the amount of information given, or uncertainty removed, when a decision is made between two equally probable alternatives: a decision made by the spin of a coin, for instance, or that made after asking 'to be or not to be?' To work in this unit, the logarithms are taken to the base 2. So the drawing of a card in the example above gives a little less information than is obtained by seeing whether a penny has fallen heads or tails.

It is an important property of Shannon's function (2) that if two or more events become indistinguishable, so that some of the E's and p's have to be combined, then the numerical value of the function always diminishes. Thus suppose, in the example above, that we no longer distinguish between E_1 and E_2. p_1 and p_2 then become the single probability $\frac{7}{26}$, i.e. $\frac{1}{52} + \frac{1}{4}$, and the quantity becomes

$$-\tfrac{7}{26}\log\tfrac{7}{26} - \tfrac{19}{26}\log\tfrac{19}{26},$$

[1] The word is inelegant, but it is already firmly established in use.

which is 0·84, i.e. 0·10 less than the previous 0·94. Conversely, if we can make a distinction where previously none was made, then the quantity will be increased.

Any quantity calculated by the formula (2) will be referred to throughout this paper as the 'amount of information,' but I use the phrase purely for convenience, and wish to imply only what is either contained in the definition or deducible from it. I shall attempt to show that this quantity, in this context, is equivalent, in its properties, to the amount of design. This is not to show that information and design are intrinsically the same, but to show that as the object of our study is changed in ways that unquestionably alter the amount of design in it, so does the quantity given by Shannon's function always change similarly. The latter can then conveniently be used as an indicator for the former, just as the height of the mercury in a thermometer-tube can be used as an indicator of the hotness of its surroundings, though 'height' and 'hotness' are by no means identical.

To apply the measure to a designed machine, we regard the machine as something specified by a designer and produced, as output, from a workshop. We must therefore consider not only the particular machine but the *ensemble* of machines from which the final model has been selected. Let me give some examples. They will illustrate the method in detail and will show that the measure agrees satisfactorily with our intuitive sense of 'amount of design.'

First suppose an electrical engineer is going to design a network of resistances. Suppose he can take for granted that there will be three resistances radiating from a point, that their values will lie somewhere between 10 and 100 ohms, and that he need not specify closer than 20 per cent. All other variations have zero probability. What he has to do is to allot to each resistance one of the values 10, 15, 22, 33, 47, 67, or 100 ohms. If the *a priori* probabilities are all equal to $\frac{1}{7}$, the amount of information is, for each resistance, $\log_2 7$ bits, and, for the whole network, $3 \log_2 7$, i.e. 8·42 bits. This number gives an exact value to the symbol D in (1).

Had a finer discrimination been necessary, the amount of information would have been larger. Thus, had each resistance to be specified to the nearest ohm, each resistance would have to be selected in value from 10, 11, 12, . . . , 99, 100 ohms, i.e. from 91 possibilities. So the amount of information would be $3 \log_2 91$, i.e. 19·52 bits. The

extra 11·10 bits (the increase from 8·42 to 19·52) represents, and measures, the extra discrimination that has been used; for it is an important property of Shannon's function that if selections are made in stages, the components combine linearly.

We can use the same function to measure the amount of information (or design) shown by the network (M of (1)). Here again we consider the possible networks, as they differ in their particular values for the resistances; we associate a probability with each distinct network, and we calculate the amount of information in the usual way. It proves to be 8·42 bits; so in this example M equals D.

This equality does not occur necessarily, and in other networks the two quantities may be unequal. Suppose, for instance, that another network, instead of being arranged as three radii, had been arranged as three resistances in parallel. Two variations that differed only by a permutation of their resistances would not be distinguishable electrically; so the designer could make variations that did not make any effective difference to the network. In such a case, M would be less than D.

On the other hand, no possible rearrangement can make M greater than D, so this first example exemplifies Descartes' dictum.

As a second example, consider the case of the designer who, to provide himself with materials, has bought a toy engineering set that contains a definite number of parts capable of being joined in a certain variety of ways. If he constructs his machine from these parts we could, with some labour, calculate the exact amount of information in his final model. Again, the variety of machines constructible cannot exceed the variety available to the designer; and it may be less if some of the forms are indistinguishable. So this second example also exemplifies the dictum. In fact, if this way of measuring 'amount of information' (or design) be accepted, the whole of information theory and all the practical experience of communication engineering becomes available to support the thesis that, in cases such as these, no possible rearrangement of parts can make the amount of information in the machine greater than that used by the designer.

3 *Dynamic Systems*

Exactly the same conclusion is reached if we ignore the form or construction of the machine and consider only its behaviour.

Every determinate machine can be specified in its behaviour by a set of ordinary simultaneous differential equations of the first order

$$\left.\begin{aligned} \frac{dx_1}{dt} &= f_1(x_1, \ldots, x_n) \\ &\cdots\cdots \\ \frac{dx_n}{dt} &= f_n(x_1, \ldots, x_n) \end{aligned}\right\} \quad . \quad . \quad . \quad (3)$$

where the f's are single valued, and where the right-hand side contains no function of t (the time) other than those whose fluxions occur on the left.[1] (For a machine is 'determinate' in its behaviour if the occurrence of a particular state determines what it will do. The equations (3) say this in mathematical form, for if the f's are given, by being appropriate to some particular machine, the occurrence of a particular set of values of the x's is sufficient to determine the changes, the dx's, that will occur during the next instant dt.) If the machine, with variables $x_1, \ldots, x_n$, is brought, at time $t = t_0$, to an arbitrary state $x_1^\circ, \ldots, x_n^\circ$ and then released, the equations, through their integrals, determine how each x will change in value with time.

When a machine is 'designed,' the designer starts with more possibilities available :

$$\left.\begin{aligned} \frac{dx_1}{dt} &= \phi_1(x_1, \ldots, x_n ; \alpha_1, \alpha_2, \ldots) \\ &\cdots\cdots\cdots \\ \frac{dx_n}{dt} &= \phi_n(x_1, \ldots, x_n ; \alpha_1, \alpha_2, \ldots) \end{aligned}\right\} \quad . \quad . \quad . \quad (4)$$

where the α's are parameters under his arbitrary control. He then selects the numerical values that they are to have, fixes them permanently, and so restricts the forms ϕ, regarded as functions of the x's, to some particular form, such as (3).

It is well known that the equations (3) define, from each point $(x_1, \ldots, x_n)$, a single trajectory. It follows that if the system (4) has k possible combinations of the α's, the machine cannot show more than k trajectories from any point, though it may show fewer. Measured in this way, the amount of information in its trajectories,

[1] W. Ross Ashby, 'The physical basis of adaptation by trial and error,' *J. gen. Psychol.*, 1945, **32**, 13-25; 'The nervous system as physical machine,' *Mind*, 1947, **56**, 1-16 ; L. von Bertalanffy, 'An outline of general system theory,' this *Journal*, 1950, **1**, 134-165

or behaviours, cannot exceed the amount primarily introduced by the designer when he decides the values to be given to the α's.

The examples given so far are thus wholly in accord with Descartes' dictum.

4 *Evolution and Design*

The question might seem settled, were it not for the fact, known to every biologist, that Descartes' dictum was proved false over ninety years ago by Darwin. He showed that quite a simple rule, acting over a great length of time, could produce design and adaptation far more complex than the rule that had generated it. The status of his proof was uncertain for some time, but the work of the last thirty years, especially that of the geneticists, has shown beyond all reasonable doubt the sufficiency of natural selection. We face therefore something of a paradox.

There can be no escape by denying the great complexity of living organisms. Neither Descartes nor Kant would have attempted this, for they appealed to just this richness of design as evidence for their arguments. Information theory, too, confirms the richness. Thus, suppose we try to measure the amount of design involved in the construction of a bird that can fly a hundred miles without resting. As a machine, it must have a very large number of parameters adjusted. How many cannot be stated accurately, but it is of the same order as the number of all the facts of avian anatomy, histology, and biochemistry. Unquestionably, therefore, evolution by natural selection produces great richness of design.

Whence comes the richness? Can it be that the rule of natural selection—'the dead shall not breed'—together with its ancillary rules, is really more complex than it seems, and is at least as complex as the design it produces? This seems most unlikely, but even if it were true the paradox would not really be resolved, for the complexity that can be generated by evolution is *independent* of the complexity in these rules. To make the argument clear and quantitative, let me give an example. The essence of the evolutionary process is selection, a single selective operator acting over and over again. Consider, then, a very simple selective machine: a magnet that selects any iron ball from a stream of balls that rolls beneath it. Let us compare the quantity of design that went to its construction with the quantity of selection that it can make, using Shannon's measure for both. When it was designed, the designer had to select its components from those

that were available to him: cogs, magnets, wires, batteries, etc.—what was available in his workshop and what could be bought with his resources. If there were A components, with equal *a priori* probabilities of being selected, the selection of a magnet involved $\log_2 A$ bits. (Theoretically, the accuracy of this measurement is limited only by our ability to give accurate definition to 'workshop,' etc.) The decision to allow balls to roll underneath involves, say, $\log_2 B$ bits; we need not enquire its exact value here. So the total amount of information put into the machine's design is $\log_2 A + \log_2 B$ bits. Now consider what the machine can achieve as a selector: if there is one iron ball in C the machine will achieve a selection of $\log_2 C$ bits. It follows that $\log_2 C$ may well be greater than $\log_2 A + \log_2 B$, for A, B and C have no necessary relationship. Similarly, however complex the specification of 'natural selection,' evolution can in time produce a greater complexity.

Information theory, however, makes clear whence comes the extra information. The law that information cannot be created is not violated by evolution, for the evolving system receives an endless stream of information in the form of mutations. Whatever their origin, whether in cosmic rays or thermal noise, the fact that each gene may, during each second, change unpredictably to some other form makes each gene a typical information source. The information received each second by the whole gene-pattern, or by the species, is then simply the sum of the separate contributions. The evolving system has thus *two* sources of information, that implied in the specification of the rules of natural selection and that implied by the inpouring stream of mutations.

It is now clear that the paradox arose simply because the words 'cause' or 'designer,' in relation to a system, can be used in two senses. If they are used comprehensively, to mean 'everything that contributes to the determination of the system,' then Shannon and Descartes can agree that 'a noiseless transducer or determinate machine can emit only such information as is supplied to it.' This formulation will include the process of evolution if the 'cause' is understood to include not only the rules of natural selection but also the mutations, *specified in every detail.* If, on the other hand, by 'cause' or 'designer' we mean something more restricted—a human designer, say—so that the designer is only a part of the total determination, then the dictum is no longer true.

This gives us what we need. The evolutionary process transcends,

in a sense, the bounds set by the dictum. What we have to do now is to develop a machine that shall, in some way, use an evolution-like process in its working.

5 *Darwinian Machinery* [1]

To develop the machine, first observe that we now have two ways of giving the instructions necessary for the production of, say, a bird. One way is to specify the details in all their complexity and individuality, filling perhaps several volumes in the process. The other way is just to write down the instructions: 'Take a planet with some carbon and oxygen; irradiate it with sunshine and cosmic rays; and leave it alone for a few hundred million years.' We may feel that this second way is somewhat irregular, but we cannot deny that it is effective.

To see what is involved, let us consider an even simpler example. Suppose we offer a prize to any inventor who, by using only 1000 bits [2] in the specification of his machine, can build one that will itself produce more than 1000 bits. Suppose that one inventor proceeds as follows. He makes a little machine, using 100 bits; he makes a Geiger counter, using 500 bits; and he then uses 1 bit to decide that the counter shall, rather than shall not, feed into the machine. He then comes to us and claims that he has built a machine that (*a*) was designed, so far as *he* was concerned, with 601 bits, and that (*b*) can emit far more than this quantity. Can we deny his claim?

He has shown us how a human being, limited by his finite intelligence so that he cannot contribute more than 1000 bits to the design, can none the less produce a machine that will emit more than this quantity. The point is that the decision to use, or not to use, a source of information—or to select one source from several—requires only a bit or two, while the source itself then contributes a quantity which may be much greater.

He has, in fact, devised an 'information-amplifier.' For what is an 'amplifier?' A power-amplifier is a device that, if given a small amount of power, will emit a larger amount. A sound-amplifier, if subjected to a small sound, will emit a larger sound. A 'money-amplifier' would obviously be a device that, if given a small amount

[1] The author is indebted to Professor Wiener for this apt adjective.

[2] Units of information, not parts.

of money, would emit a larger amount of money. In all cases the output is of the same quality as the input, but the input is not used to provide part of the output : it is consumed in controlling the flow of 'material' from a copious source, or large reservoir, of the same 'material' ; and it is the reservoir that provides the output. Exactly the same method was used by the inventor : his 601 bits were used, not to provide part of the output but to control the flow of fresh information from a much larger source.

At this point the critic may well object that such information, being unorganised and chaotic, is useless. To this I would reply that chaotic information is by no means useless, but is, in fact, perfectly usable (as evolution has shown by its use of mutations) *provided that the machine has been designed to make the necessary selection.*

The main lines of the machine that is to go past the limit set by Descartes' dictum is now clear. The designer should make it selective, like the 'magnet-machine' considered earlier, and he should couple it to an abundant source of information such as a Geiger counter or a device using thermal noise. The amount of design needed to specify the machine's construction, and the amount of information that it can emit as output, will then be dissociated, and the amount that can be emitted is no longer limited to being equal to, or less than, the amount used in the design. If P bits went to its construction, and if it selects something as rare as 1 in Q, then if Q is greater than 2^P the designer can claim that the machine is showing more design than he has put into it.

6 *The Homeostat*

The argument may be made clearer by now turning to the homeostat, which was built precisely to admit and to use unorganised information.[1]

First, what is the homeostat ? Here we need its abstract functional, rather than its practical, details.

In a sense it is a machine within a machine, so its description is best taken in two stages. The primary machine consists of four Units, all of which act on each other, and each of which bears on its top a needle whose deviation from the central position is the focus of

[1] W. Ross Ashby, in *Electronic Engineering*, 1948, **20**, 379–383. (A full account of the machine and of its principles in relation to the living brain, especially with regard to the origin of adaptive behaviour, is given in my *Design for a Brain*, to be published shortly by Chapman and Hall, London.)

interest. The four deviations (x_1, x_2, x_3, x_4) depend on sixteen parameters a_{ij} in accordance with the equations (of the same form as (3) above)

$$\left.\begin{array}{l} \dfrac{dx_1}{dt} = a_{11}x_1 + a_{12}x_2 + a_{13}x_3 + a_{14}x_4 \\ \qquad \cdots\cdots\cdots \\ \dfrac{dx_4}{dt} = a_{41}x_1 + a_{42}x_2 + a_{43}x_3 + a_{44}x_4 \end{array}\right\} \quad . \quad . \quad . \quad (5)$$

Its only resting state is at the origin, where it can be in either stable or unstable equilibrium.

If the latent roots of the matrix $[a_{ij}]$ have real parts that are all negative, the system will be stable. Its needles will then move to the central position and, if displaced, the Units will so interact that a co-ordinated movement brings the needles back to the centre. If one or more of the real parts is positive, the system will be unstable, 'vicious circles' of action will develop, and the needles will move away from the centre. To this extent the machine is simply an analogue computor, for if the values of the parameters a_{ij} are given, its behaviour provides integrals of the equations (5).

It has, however, further components which influence this first activity. If any one of the x's goes outside certain bounds (a deviation of more than about $\pm 45°$) the extra components act and change the a's in the corresponding row to a new set of values that are taken at random from a distribution that lies evenly between -9 and $+9$. The actual values came originally from a table of random numbers.[1] So long as the needle stays outside the bounds, so long do the a's keep changing, at about one-second intervals. As soon as the needle returns to within the bounds, the a's stop changing and retain their latest set of values.

When set working it behaves in the following way. If stable, the needles come to the centre ; it will then demonstrate, if disturbed, the co-ordinated response typical of a stable system. If, however, it is unstable, the needles diverge, and the a's undergo substitution after substitution. The first stable combination of a's to arrive is retained. Whether a particular combination will be stable depends partly on what other conditions (the 'problem') have been imposed on the mechanical and other parts of the system (for commutators can reverse

[1] R. A. Fisher and F. Yates, *Statistical Tables*, Edinburgh, 1938

the normal polarities of connection, a needle or two can be locked, needles can be tied together, and so on, making the equations (5) more complicated than is shown). Its behaviour can therefore be described by saying that it hunts for, and retains, a matrix that is stable in the imposed conditions. If the conditions are changed, so that the matrix is no longer stable in these new conditions, it will at once hunt for, and eventually retain, a new matrix that gives stability in the new conditions.

Its process is clearly similar to that occurring in evolution. There the rules are: test the organism against the environment; if the organism is unfit remove it; replace it by a new organism that differs from it in some random way. In the homeostat the rules are: test the matrix for stability in the imposed conditions; if it is unstable remove it; replace it by a new matrix with random elements. In both, the new material varies merely randomly from the old.

The homeostat was built before Shannon's theory of information had been published. It is instructive now to apply the theory to the homeostat's design. Let us consider the original model. There are four uniselectors (stepping-switches) and each can take, independently, one of twenty-five positions. The amount of information in its output is thus $4 \log_2 25$, i.e. 18·6 bits. The amount that went to its design can be very roughly estimated at 200–800 bits. It seems, therefore, that though the homeostat, in principle, can claim to produce more design than was put into it by the designer, the original model cannot so claim—its reservoir of new information is too quickly exhausted. Its amplification-factor is almost ludicrously below unity.

This deduction is somewhat sobering, but from it come two reflections. We can see how powerful is Shannon's theory in its quantitative grasp of these questions, and we can also notice, what the evolutionary process itself suggests, that a very great deal of selection may be necessary if Descartes' limit is actually to be passed.

7 *Conclusion*

It seems now to be clear that while Descartes' dictum is true if by 'designer' we mean 'every detail that contributes to the machine's performance,' it is not true if by 'designer' we mean 'the man who specifies its construction.' To defeat the limit implied by the dictum, the designer must include, as one of his specifications, 'admit other

information.' Stated thus the method may seem to be mere trickery, on a par with that of the boy who, given three wishes, made one of them a request for three more wishes. Trickery or not, however, it offers the designer a practical method for overcoming the limitations of his own powers of design.

Once the method has been identified, we can see that it is not unreasonable. We can see, for instance, that the more minutely we design a machine to play chess just as we want it to—admitting no other information—the more certain it is to play just *our* sort of chess, with all our faults and wrongly-conceived strategies. We are, in fact, in exactly the same position as the father, a keen but mediocre chess-player, who wants his son to become world champion. It is true that the father should teach the child, but he must not teach his son every reply in detail lest he limit the son's play to being the merest replica of the father's. If the father really wants the son to beat him he must sooner or later stop telling the son what to do and must send him out into the world to be subjected to all sorts of unselected experiences. The understanding father will not try to teach his son all chess but will try to teach him how to profit by future experiences.

Designing a machine has much in common with teaching a child, for in each case the almost infinite possibilities have to be reduced to a selection. Were Descartes' dictum to be stated in the form ' no child can know more than he has been taught ' we would at once see the equivocation, for the teacher can not only teach the child facts but can also teach him how to use the ' free ' information in the world, and thus how to surpass his teacher. If we wish to build a machine that can beat us at chess, or to build a ' real ' brain, we must follow the same method ; we must aim in design, not a machine that will play chess, but at a machine that can make trials, and select. The homeostat was intended to be a first step in this direction.

8 *Summary*

The quantitative aspects of the question ' how much of a machine's behaviour is attributable to the designer ? ' can be treated accurately and meaningfully by the methods of information theory. Its application leads to the deductions :

It is quite possible for a mechanical chess-player to outplay the man who designed it.

For this to be possible, the designer must construct the machine so that it can receive and use information not provided by him in detail.

The extra information need not be organised or intelligible—that given by thermal noise may be sufficient.

(An examination of the homeostat illustrates the method in detail.)

Department of Research
Barnwood House
Gloucester

THE PREHISTORY OF RESEARCH INTO FOUNDATIONS*

EVERT W. BETH

1 *Introduction*

IN two previous articles [1] I have endeavoured to show that modern research into the foundations of mathematics and physical sciences has upset several doctrines which are important for traditional philosophy, especially Aristotle's theory of science. It was pointed out that this situation explains the bitter controversies between representatives of modern research into foundations and defendants of traditional speculative philosophy. But modern research into foundations does not compel us to take a purely negative attitude with regard to philosophical tradition. On the contrary, modern conceptions can often help us to obtain a better insight into various features of ancient philosophy, and it is the purpose of the present article to make this clear.

It may appear that this purpose is inconsistent with the conclusions set forth in my earlier papers. But we must not forget that Aristotle's theory of science itself constituted the result of a historical development; it was intended to provide an answer to the problems which had arisen from the growth of mathematics and physical science during the relatively brief period which immediately preceded the flourishing epoch of Attic philosophy.

So long as Aristotle's theory of science was accepted as a matter of course, his answers to these problems were also taken to constitute their final solution and, therefore, the interpretation of pre-Attic philosophy was strongly influenced by peripatetic views. It is true that during the last century classical scholars have become more and more aware of this fact and have done their utmost to liberate the study of pre-Attic philosophy from the often detrimental influence of Attic thought. Nevertheless, the development of research into foun-

* Received 11. vii. 51

[1] 'Critical Epochs in the Development of the Theory of Science,' this *Journal*, 1950, **1**, 27; 'Fundamental Features of Contemporary Theory of Science,' *ibid*. 291.

dations has itself opened the way for new ideas in the interpretation of pre-Attic philosophy, for it made clear that in many cases Aristotle's answers to the problems treated by pre-Attic and Attic philosophy did not, as earlier generations had believed, provide a final solution of them. Many of these problems turned out to be still open to discussion and the answers given by Aristotle's predecessors turn out, in several instances, to present considerable interest from a modern point of view. Moreover, several features of Aristotle's own philosophy appeared also in a new light. Accordingly, a new direction in the study of ancient philosophy was initiated in the work of I. M. Bocheński, Ph. Boehner, K. Dürr, F. Enriques, J. Łukasiewicz, B. Mates, K. R. Popper, L. Rougier, Bertrand Russell, H. Scholz and others who showed that often a more accurate insight into old ideas could be obtained in the light of new experience. I venture to sum up in the following pages the main results of my own studies on the subject, which were published in a series of books and papers during the years 1941–48, but which so far have remained, for various reasons, difficult of access to most scholars outside the Netherlands.[1]

2 *The Concept of Nature in Ancient Philosophy*

It has been argued by H. Kelsen[2] that the widespread and deep-rooted belief in the so-called Law of Causality derives from a Principle of Compensation which is found already in men still unaware of causality in our sense ; therefore, the philosophy of nature owes its

[1] E. W. Beth, 'Gorgias van Leontini als wijsgeer,' *Alg. Ned. Tijdschr. v. Wisbg.*, 1941–2, **34** ; *De wijsbegeerte der wiskunde van Parmenides tot Bolzano*, Antwerpen, 1944 ; *Geschiedenis der logica*, Den Haag, 1944 ; 2nd ed., revised, 1948 ; *De strekking en het bestaansrecht der metaphysica in verband met de toekomst der wijsbegeerte* (Inaugural Lecture), Groningen, 1946 ; 'Historical Studies in Traditional Philosophy,' *Synthese*, 1946, **5** ; 'The Origin and Growth of Symbolic Logic,' *Synthese*, 1948, **6** ; 'La cosmologie, dite naturelle, et les sciences mathématiques de la nature,' *Archives de l'Institut International des Sciences Théoriques*, 1948, A I **1** ; 'Les relations de la dialectique à la logique,' *Dialectica*, 1948, **1** ; 'Deux études de philosophie grecque,' *Philosophical Essays*, Library of the Xth International Congress of Philosophy, vol. 2, Amsterdam, 1949

[2] H. Kelsen, *Vergeltung und Kausalität*, The Hague, 1941 ; Eng. trans. *Society and Nature*, Chicago, Ill., 1943. Cf. Gregory Vlastos, 'Equality and Justice in Early Greek Cosmologies,' *Classical Philology*, 1947, **42** ; this author, apparently without knowing Kelsen's work, defends an entirely similar interpretation of Greek cosmology. His paper contains interesting material which is mentioned neither by Kelsen nor in the present article. But I think that he is not justified in attributing to Democritus a tendency to naturalise and humanise the concept of justice ; cf. p. 60, nn. 8, 9.

origins to an underlying older philosophy of society. I shall show that Kelsen's thesis receives strong support from a number of Greek philosophical texts and that it can contribute to a better understanding of several of them.

Kelsen's thesis is illustrated by the conception of *harmony*, which plays such an important rôle in Greek thought. Both the cosmic law and the social law are intended to maintain (cosmic and social) harmony; such a law (*a*) determines the normal course of events, and (*b*) fixes a compensation for every violation of the normal course of events, in order to restore the harmony which, by this violation, has been menaced. These conceptions are defended by Heraclitus against dissidents: 'They fail to understand how, being at variance, it (i.e. cosmic order) reconciles itself; a harmony of opposite tendencies.'[1] The same view is elsewhere expressed as follows: 'The opposites come to unity and out of the divergence arises the most splendid harmony.'[2] Similar views are even more explicitly stated by Philolaos: 'The equal and related had no need for harmony, but the unequal and unrelated and unequally endowed must necessarily be embraced by such a harmony that it can be retained within a cosmos.'[3]

Though there is, even in Heraclitus, a tendency to distinguish between cosmic law or *φύσις* and social law or *νόμος*, their relatedness and analogy are repeatedly stressed. 'The *φύσις* likes to hide itself.'[4] 'Hidden harmony (i.e. divine, natural, order) is better than manifest harmony'[5] (i.e. artificial, man-made, order). 'The *φύσις* suffices for all in all' (i.e. social law is superfluous).[6] The meaning of the last fragment is explained elsewhere: 'For it (i.e. cosmic law) reigns as much as it wishes and it suffices for all and surmounts all.'[7] A comparison with a few texts by Democritus and Philolaos will give some idea of the importance of sociomorphic views for pre-Socratic philosophy in general. 'Chance is generous, but the *φύσις* is self-sufficient'[8] (i.e. chance is partial, but cosmic law can afford to be just). 'Injustice is opposite to the *φύσις*.'[9] 'As to the *φύσις*, this is as follows: the essence of things, which is eternal, and the *φύσις* itself demand a

[1] Heraclitus 22 B 51. For pre-Socratic references see Diels-Kranz, *Die Fragmente der Vorskratiker*, 5th ed., 1934.

[2] Heraclitus 22 B 8

[3] Philolaos 44 B 6

[4] Heraclitus 22 B 123

[5] Heraclitus 22 B 54

[6] Heraclitus 22 C 2

[7] Heraclitus 22 B 114

[8] Democritus 68 B 176

[9] Democritus 68 A 69

divine and not a human thinking.'[1] 'Neither the φύσις of number nor the harmony admit any lies.'[2]

Very significant is the combination of the terms φύσις and ἀνάγκη in the expressions ἀνάνκη φύσεως and φύσις ἀναγκαία, which are found also in Greek tragedy[3]; the views underlying Greek tragedy were apparently closely related to the principles of sociomorphic cosmology. Even more important is the expression δικαίη φύσις, 'just phusis,' which is used in the Corpus Hippocraticum to denote the normal state of the organism; its opposite is βίαιον. It was Antiphon[4] who synthesised all these conceptions, starting from the notion of an ἀνάγκη φύσεως. Everything, man included, is dominated by inflexible cosmic laws; whoever violates these laws, cannot escape the terrible consequences. We noted already the influence of these views on Greek tragedy. When we now turn to Plato and Aristotle, it is not difficult to show their acceptance of sociomorphic doctrines. Expressions such as βία, βίαιος, βιάζω, denoting a violation of cosmic law, are frequently used. In Aristotle we find the term βίᾳ, 'with violence,' and, more frequently, κατὰ φύσιν, παρὰ φύσιν, 'in accordance with cosmic law'—'contrary to cosmic law.'

Straton of Lampsacus seems to have been the first to defend a view which comes considerably nearer to modern conceptions. 'He (Strato) says that the cosmos is not a living being and that the κατὰ φύσιν comes near to the κατὰ τύχην; for chance provides for the start, and then each of the physical effects is produced accordingly.'[5] Straton may have been influenced by Epicurus, but it is not necessary here to dwell on this point.

The Stoics return to the sociomorphic conception of the universe to which they often give a striking expression. 'Destiny is the moving force of matter, it always remains the same and can be called both providence and φύσις.'[6] 'Destiny is the unifying cause of things or the logos according to which the cosmos is governed.'[7] These texts were by Zeno and Stoic. Chrysippus, in the following statements, is even more explicit. 'Zeus is called also the universal

[1] Philolaos 44 B 6
[2] Philolaos 44 B 11
[3] F. Heinimann, *Nomos und Physis*, Basel, 1945
[4] F. Heinimann, loc. cit.
[5] Plutarchus, *Adversus Colotum* 14, 1115 A
[6] Aëtius I, 27, 5; H. Diels, *Doxographi Graeci* 322 b 9
[7] Diogenes Laërtius, *De vitis*, etc., VII, 149

φύσις of all things, and destiny and necessity.'[1] 'Our φύσεις are the parts of the φύσις of the whole . . . the universal law.'[2] 'This cosmos is the state writ large and it depends on one single constitution and on a law. And there is a word of the φύσις, which prescribes what is to be done and forbids what is not to be done.'[3] 'It is impossible to find for justice another principle or another origin than the one from Zeus or from the universal φύσις.'[4] 'The aim is to live according to the φύσις, that is, in accordance with both that of oneself and that of the cosmos. By the φύσις, according to which one must live, Chrysippus means both the universal and the specifically human . . .'[5] I do not think that these texts demand many comments. The introduction of a specifically human φόσις in addition to the universal φύσις, seems to be due to Chrysippus himself; it seems probable that it was intended to provide a basis for the social law or νόμος.

The Greek sociomorphic conception of the universe proves to be still alive in the work of Alanus de Insulis (Alain de Lisle), a Flemish philosopher living in the twelfth century.[6] An interesting revival of the ancient sociomorphic cosmology is found in the statement, by R. W. Emerson, of a Universal Law of Compensation. In a broader sense, we can say that modern determinism has its roots in deeply religious conceptions which flourished in Antiquity. Kelsen was completely right in defending the thesis that, strange though it may seem, determinism derives from the primitive man's conception of social law as a divine institution operating independently of any voluntary human effort and from his tendency to describe natural phenomena in social terms. Kelsen for the first time correctly described the sociomorphic cosmology which derives from this primitive conception of society and nature. We may hope that in the near future his ideas will contribute considerably to our understanding of ancient thought.

[1] Philodemus, *De piëtate* c. 11 ; H. Diels, *Doxographi Graeci*, 545 b 31

[2] Diogenes Laërtius, *De vitis*, etc., VII, 87

[3] Philo, *De Joseph*, vol. 2, ed. Mangoldt, p. 46

[4] Plutarchus, *De Stoicorum Repugnatis* 9, 4, 1035

[5] Diogenes Laërtius, *De vitis*, etc., VII, 87 ss.

[6] E. J. Dijksterhuis, *De mechanisering van het wereldbeeld*, Amsterdam, 1950, who quotes : M. Baumgartner, *Die Philosophie des Alanus de Insulis im Zusammenhang mit den Anschauungen des 12. Jahrhunderts dargestellt* (in *Beiträge zur Geschichte der Philosophie des Mittelalters*, vol. 2, part 4), Münster, 1896

3 *Unity and Plurality in the Philosophy of the School of Elea*

After displaying the possibilities offered by a new approach to the problems inherent in the interpretation of Greek philosophy, I now turn to the central theme of this article, namely, the analysis of Greek abstract thought in the light of modern achievements in logic and mathematics.

It is beyond dispute that Parmenides, in ontology, attained a level of abstraction which considerably transcends that of any of his predecessors or contemporaries. There is, however, much difference of opinion as to exactly how abstract his conceptions were. From the texts available at present we do not get an explicit answer to the question, whether or not Parmenides conceived Being as spatially extended. In fact, several statements by Parmenides himself strongly suggest a spatial extension of Being which seems hardly consistent with his thesis of the Unity of Being. Nor does the doxography provide a definite and satisfactory answer. Accordingly, modern scholars defend strongly divergent views.[1]

Judging by the scarce fragments now available, Parmenides' first followers already seem to have defended opposite interpretations of his doctrine. Zeno accepted the spatial extension and the divisibility of Being, which were both denied by Melissus.[2] This difference of opinion had important historical effects. It provided the sophist Gorgias with the arguments to refute Parmenides' philosophy. He first used Melissus' argument against the divisibility of Being, and then borrowed Zeno's refutation of its indivisibility. Plato accepted Melissus' view in his *Theaetetus*, but in his *Parmenides* and his *Sophist* it is Zeno who appears as the spokesman of the master. It seems that the different interpretations of Parmenides' doctrine, found both in ancient and in modern literature on the subject, trace back to the dispute between Melissus and Zeno, his first adherents. Therefore, the only adequate manner to approach our problem is to begin by trying to settle their dispute. Once this task has been fulfilled, it will be much easier to handle later views on the matter.

At first sight, it may appear that only Melissus' thesis of the indivisibility of Being is consistent with the general tendency of Parmenides' thought and with his own words: 'Thinking shall

[1] Zeller, Tannery, and Diels ascribe to Parmenides a spatial conception of Being, which interpretation is rejected by Brandis, Natorp, and Kinkel.

[2] Zeno 29 A 21, 29 B 1; Melissus 30 B 9

not disjoin Being from its connection with Being.'[1] But we must distinguish two kinds of division: a division which destroys the connectedness of the parts of the divided entity, and a division which lets this connectedness subsist. What Parmenides wanted to deny is, in my opinion, merely the possibility of subjecting Being to a division of the first kind. Melissus' conclusion, which I consider as erroneous, that Parmenides' doctrine is inconsistent with the thesis of the divisibility of Being, apparently derived from his overlooking division of the second kind. This implies that Melissus must have imagined any division of Being to be like the breaking of a stick. Now such an oversight is quite plausible in a man whose conception of Being, according to Aristotle, was more materialistic than Parmenides.'[2]

It seems to follow that Zeno's interpretation of Parmenides' views can be brought to agree with the master's intentions, provided the division of Being which Zeno had in mind was a division of the second kind. Again, it is extremely plausible that this was indeed the case. Not only may we assume that Zeno has been familiar with geometrical constructions, such as the bisection of an angle, which constitute divisions of the second kind; we actually know that these divisions have played an important rôle in his philosophy, namely, in his derivation of the paradoxes of continuity and movement.

This discussion on the philosophy of Parmenides, which seems to warrant the conclusion that both the spatial extension of Being and the possibility of a division (of the second kind) are contained in or at least consistent with his ontology, brings us immediately to another problem: what did Zeno mean when he stated his famous paradoxes? I fully agree with Tannery,[3] that Zeno intended to refute the Hypothesis of Plurality. This hypothesis must be taken to state the possibility of dividing the continuum into parts (atoms) which are indivisible and incoherent. It follows that Zeno's refutation of the Hypothesis of Plurality was closely connected to his controversy with Melissus. As a matter of fact, one of the obvious conclusions from Zeno's paradoxes is the impossibility, for the continuum, of a division of the first kind.[4]

[1] Parmenides 28 B 2; cf. 28 B 8; the first text seems to be directed against Anaximenes 13 A 6.

[2] Melissus 30 B 10; Aristotle, *Metaphysica* A 5, 986 b 18

[3] P. Tannery, *Pour l'histoire de la science hellène*, Paris, 1887

[4] I agree with B. L. van der Waerden ('Zenon und die Grundlagenkrise der griechischen Mathematik,' *Mathematische Annalen*, 1940, **177**) that there are no

Our main sources for Zeno's paradoxes are Aristotle's well-known summary and Simplicius' comments on it.[1] It seems, however, that so far hardly sufficient attention has been paid to the much more elaborate account which is found in Sextus Empiricus.[2] Sextus' source is a work by Diodorus Cronus which seems to have consisted of an amplified version of Zeno's argumentation. Now Sextus not only explicitly states the Hypothesis of Plurality, but also mentions a consequence not stated by Aristotle : if the Hypothesis of Plurality were true, that is, if space and time consisted of atoms, then there is only one possible speed, namely, a speed of one space atom *per* time atom. This conclusion, which Zeno may have derived from his Achilles paradox, provides at once a clue to the meaning of the difficult Stadion paradox. Suppose we have two particles, moving in opposite directions, each with a speed of one space atom *per* time atom ; then their relative motion will have a speed of two space atoms *per* time atom. Such a speed, however, has been shown to be impossible ; hence even a speed of one space atom *per* time atom is impossible. It follows that movement is impossible, provided the Hypothesis of Plurality is true.[3]

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grounds to attribute, with H. Hasse-H. Scholz ('Die Grundlagenkrisis der griechischen Mathematik,' *Kantstudien*, 1922, **33**) a system of infinitesimal calculus *à la* Cavalieri to Zeno's opponents ; we must rather ascribe to them an atomistic conception of the continuum comparable to the views of Giordano Bruno ; cf. H. Lasswitz, *Geschichte der Atomistik*, Hamburg-Leipzig, 1890, vol. I.

[1] All these texts are reproduced, of course, in Diels-Kranz, *Die Fragmente der Vorsokratiker* ; cf. footnote 1 on p. 60. H. D. Lee, *Zeno of Elea*, 1936, Cambridge, gives the texts of Zeno's arguments with a translation and valuable comments. The author rightly stresses the divergences between Melissus and Zeno (p. 113) and clearly points out that Aristotle, in his discussion of the Stadion paradox, 'has entirely missed the point of the argument' (p. 89). But, in my opinion, his defence of the conventional view 'that Parmenides had declared all plurality and motion to be in some way or other illusory,' and 'that Zeno's arguments were intended to give support to Parmenides' thesis' (p. 65) against the interpretations set forth by P. Tannery and G. Milhaud, is not convincing.

[2] Sextus Empiricus, *Pyrrhoniae institutiones*, III, 76–81 ; *Adversus physicos*, II, 119–68, in particular II, 154.

[3] Sextus' account has been used by V. Brochard, *Etudes de philosophie ancienne et de philosophie moderne*, Paris, 1900. The author arrives at an interpretation similar to the one defended in this article. F. M. Cornford, *Plato and Parmenides*, 1939, London, 58–9, gives an adequate description of the philosophical position which was attacked by both Parmenides and Plato ; it seems to follow from the evidence presented in this article that Melissus shared some of the views of these opponents of Eleatic philosophy.

4 *Aristotle's Principle of the Absolute*

A considerable number of arguments in speculative philosophy are based on a certain principle, which is, in most cases, tacitly assumed. This principle has been applied with remarkable virtuosity by Aristotle, and will be called the Principle of the Absolute. It can be stated as follows :[1] *Suppose we have entities u and v and let u have to v the relation F ; then there is an entity f which has the following property : for any entity x which is distinct from f, we have* (i) *x has the relation F to f, and* (ii) *f has not the relation F to x.* The entity *f* will be called the *absolute entity* corresponding to the relation *F*. By introducing symbols $F(x, y)$, meaning : ' x has the relation F to y,' we can give our statement of the Principle of the Absolute the form : *If for certain entities u and v we have F(u, v), then there is an entity f such that for any entity x, we have : if x ≠ f, then F(x, f), but not F(f, x).* By using the notation of symbolic logic, we obtain :

$$(Eu)(Ev)F(u, v) \rightarrow (Ef)(x)[\textit{if } x \neq f, \textit{ then } F(x, f) \ \& \ \overline{F(f, x)}].$$

The following examples, chosen from traditional philosophy, show typical applications of the Principle of the Absolute.

(1) Let $F(x, y)$ be the phrase : x takes its origin from y ; then f will be the ' principle ' (ἀρχή) in the sense of pre-Socratic philosophy.

(2) Let $F(x, y)$ be the phrase : x is moved by y ; then f will be the ' first motor ' in the sense of Aristotle.[2]

(3) Let $F(x, y)$ be the phrase : x is desired for the sake of y ; then f will be the ' summum bonum ' in the sense of Aristotle.[3]

In this example, I will reproduce Aristotle's argument, as it provides a typical illustration of the manner in which the Principle of the Absolute is usually applied :

> Every art and every way of enquiry, and likewise every activity and every deliberation aims, it seems, at some Good ; hence it has been very rightly said that the Good is that at which everything aims.

[1] Aristotle, *Metaphysics* A 2. The following observations have, to some extent, been anticipated by F. Enriques, *Problemi della scienza*, Bologna, 1906 ; L. Rougier, *Les paralogismes du rationalisme*, Paris, 1920 ; and B. Schultzer, *Transcendence and the Logical Difficulties of Transcendence*, Copenhagen-London, 1936. The statement of the principle which is given in the present article seems to be more precise and more general than the statements given so far.

[2] Aristotle, *Metaphysics* *Λ* 7, 1072 b 25

[3] Aristotle, *Ethica Nicomachea* A 1, 1094 a 18

> Now it might seem necessary to make a distinction among our aims ; for some of them coincide with our activities themselves, other ones with some result beyond these latter. In those cases where there are aims beyond the actions, it appears that the results are more important than the activities. . . . For these latter are pursued merely for the sake of the results. As a matter of fact, it does not make any difference whatever whether the activities themselves or something beyond them constitute the aims of our actions. . . . Now if there is for our actions some aim which we desire in itself, other aims being pursued merely for the sake of this one, and if it is not the case that we want all things because of something else (for this would involve us in a *regressio in infinitum* and hence all our endeavours would become vain and empty), then it will be clear that the aim of all our activities must be the Good and even the Supreme Good.

Aristotle tries to deal with three objections (which, *mutatis mutandis*, can also be made against other applications of the Principle of the Absolute). First, there are activities which are enjoyable in themselves and which we pursue without thinking of any purpose beyond the pleasure we derive from them ; secondly, there are activities with a very definite purpose which in our eyes constitute a sufficient reason for pursuing them. The (implicit) answer to these objections seems to be that activities of this kind are either futile or they tend to some more important purpose of which we are unaware. For instance, we think we walk for pleasure, but in reality walking is good for our health ; we take medicine to restore our health. But in both cases, by maintaining or restoring our health, we contribute to our happiness. The third objection ties on to this answer. In the second case, the purpose we had in mind turned out to be subordinate to another purpose of which we are usually unaware ; why should not this higher purpose again be subordinate to a still higher purpose, and so on? Aristotle's answer to this objection is not cogent. So long as we remain in the realm of our conscious intentions, there is no *regressus in infinitum*. The purposes of which we are aware may depend on an infinite series of purposes of which we are unaware, but this does not matter if the purpose we have in mind is a sufficient stimulus for action.

(4) Let $F(x, y)$ be the phrase : x is in a certain state of movement with regard to y ; then f will be Newton's absolute space.

(5) Let $F(x, y)$ be the phrase : the imperative x is based on the imperative y ; then f will be the categorical imperative in the sense of Kant.

(6) Let $F(x, y)$ be the phrase : x takes its exchange-value from y ; then f will be the ' Wertsubstanz ' in the sense of Marx.

The application of the Principle of the Absolute is what Plato [1] calls 'progressing to the anhypotheton' and what Aristotle [2] calls 'induction.' Belief in the Principle of the Absolute is the basis for several of the traditional proofs for the existence of God. Kant [3] refers to the Principle of the Absolute as the 'Prinzip der Vernunft.' In his opinion, the principle has no conclusive force. But he observes that the human mind has a strong inclination to rely on it; therefore he thinks that philosophy should accept it as a heuristic principle. As a matter of fact, the Principle of the Absolute is not a logical identity; we can prove this easily by giving a counter-example, that is, by indicating a relation F for which the principle does not hold. Let $F(x, y)$ be the phrase: the segment x is greater than the segment y. Then it follows from the Principle of the Absolute that there must be a segment f which is smaller than any other segment. On the other hand, it is well known that there cannot be any segment f which has this property. It follows that the Principle of the Absolute does not hold for the relation F under consideration. Hence this principle cannot be a logical identity.[4]

It follows that the unrestricted application of the Principle of the Absolute may lead to incorrect conclusions. So Kant was undoubtedly right in observing that it cannot be considered as constituting, in itself, a reliable instrument of proof. But in special cases conclusions drawn from the Principle may be correct.

5 *Plato's Theory of Ideas*

It is interesting to note that Plato's Theory of Ideas can be derived from the Principle of the Absolute. Let A be any property, and let $F(x, y)$ be the phrase: the fact that the entity x has the property A presupposes the fact that the entity y has the property A. Let a be the absolute entity corresponding to the relation F so defined; then we call a also the absolute entity corresponding to the property A. It will be clear that in this special case we can give the Principle of the Absolute the following form:

[1] Plato, *Republic*, 511 B

[2] Aristotle, *Topica*, A 12, 105 a 13

[3] I. Kant, *Kritik der reinen Vernunft*, A 307–8, 1 669, A 671

[4] From his discussion of Zeno's paradoxes (cf. footnote 1 on p. 65), it is obvious that Aristotle was fully aware that there cannot be a smallest segment. He apparently did not consider the consequences of this fact with regard to the Principle of the Absolute.

Suppose we have an entity u which has the property A; then there is an entity a which has the following property: for any entity x distinct from a, we have (i) *the fact that x has the property A presupposes the fact that a has the property A, and* (ii) *the fact that a has the property A does not presuppose the fact that x has the property A.* Or, if we introduce symbols $A(x)$, meaning 'x has the property A,' we have the form:

If, for a certain entity u, we have $A(u)$, then there is an entity a such that for any entity x, we have: if $x \neq a$, then $A(x)$ presupposes $A(a)$, but $A(a)$ does not presuppose $A(x)$.[1]

The absolute entity a may be construed to correspond to what Plato called εἶδος or ἰδέα. Therefore we call the principle, which has just been derived as a special case of the Principle of the Absolute, the Principle of the Idea.

The acceptance of this principle gave rise to the famous Problem of Universals. Suppose the property A is one which is usually predicated of corruptible entities; let us suppose, moreover, that the absolute entity a is also corruptible. Then the destruction of the entity a would obviously cause every other entity x to lose the property A. This is a disturbing consequence of our supposition. So it seems that we must suppose absolute entities not to be corruptible (*genus non perit*). But then the question arises, what are the connections between corruptible and absolute entities?

Plato's first solution of this problem is as follows: As the absolute entity a, corresponding to the property A, cannot be identical with any of the corruptible entities having the property A, it seems to follow that the absolute entity a is an entity of an entirely different kind, existing separate from any corruptible entity (theory of separation). This theory, however, has consequences which seem hardly acceptable. Let A be any property and let a be the corresponding absolute entity. We can now define a new property B to mean 'having the property A and being, moreover, distinct from the absolute entity a.' If the theory of separation is true, then every corruptible entity x which has the property A must also have the property B, and conversely; the absolute entity a, however, cannot have the property B. It follows that the absolute entity b, which corresponds to the property B, must be distinct from a. We now define a property C to mean 'having the property B and being, moreover, distinct from the absolute entity b.' Again, every corruptible entity

[1] I do not give a statement by means of logical symbols, as this would involve the introduction of modal operators.

x which has the property A will have the property C, but neither of the absolute entities a and b can have the property C. It follows that the absolute entity c, which corresponds to the property C, must be distinct both from a and from b, and so on.

So we may conclude that, if the theory of separation is true, then every property A which can be predicated of corruptible entities demands the existence of an infinite series of absolute entities a, b, c, . . . , each of which exists separate from the corruptible entities having the property A.[1] Aristotle therefore proposes an alternative solution of the Problem of Universals. As the absolute entity a cannot be supposed to exist separate from the corruptible entities which have the property A and as, moreover, it cannot be identical with any one of these entities, it seems to follow that the entity a is not numerically distinct from these entities but exists somehow in them (theory of inherence : *praedicatum inest subjecto*).

This theory also turns out to have its weak spots. It seems to imply that the absolute entity a is, in a sense, spread over the entities x which have the property A ; and this implication leads to puzzling conclusions. For, first : the theory of inherence was intended to avoid the multiplication of absolute entities ; but now we meet again with a multitude of entities, namely, the different parts of the absolute entity a which inhere in each of the entities x having the property A. Secondly : how can these *different* parts of the absolute entity a convey *the same* property A to each of the various entities x in which they inhere ? And thirdly : what becomes of a part of the absolute entity a when the entity x in which it inheres happens to be destroyed ? These and similar problems are discussed at length in Plato's *Parmenides*, which might well reflect Plato's discussions with Aristotle on the subject.

Modern logic has no difficulty in showing that Plato's Principle of the Idea, if handled adequately, does not, intrinsically, give rise to any logical difficulties. As a matter of fact, the (weak) ϵ-axiom which was introduced by Hilbert[2] can be interpreted as a new version of this principle. It is true that in modern logic it is not usual to

[1] Aristotle, *Metaphysics* Z 6, 1032 a 2–4 ; cf. A 9, 991 a 12–14. For a thorough discussion of all texts concerning this so-called paradox of the third man, cf. A. Spielmann, *Die Aristotelischen Stellen vom τρίτος ἄνθρωπος*, Paris, 1891. F. M. Cornford, *Plato and Parmenides*, 1939, London, pp. 87 ff., also gives much interesting material for this argument.

[2] D. Hilbert–P. Bernays, *Grundlagen der Mathematik*, Berlin, 1938, vol. 2

take explicitly into account the occurrence of corruptible entities. But it has been shown by Bolzano [1] that we may give an interpretation of sentences involving a time determination without referring to corruptible entities.

6 *Plato's Theory of Ideal Numbers*

Plato himself attempted to solve the difficulties raised by the theories of separation and inherence by establishing an entirely new theory which is known as the Theory of Ideal Numbers. I will try to reconstruct the historical background of this theory before giving a summary of the theory itself as it has been handed down to us.

I have shown that a primitive atomistic theory of the continuum had been upset by Zeno's paradoxes. After the discovery of irrational proportions it became clear that no repair of the atomistic theory was possible. So the conception of a line as composed of its separate points broke down, and the working mathematician was deprived henceforth of a very valuable tool.

Now there is a striking similarity between the paradoxes of the continuum and the paradoxes of universals. A line cannot be considered as consisting of its separate points, but neither can it be considered as existing separate from its points ; likewise, an absolute entity cannot be considered as consisting of the separate entities having the property to which it corresponds (or as consisting of parts inherent in each of these entities), but neither can it be considered as existing separate from these entities. We can express the similarity between the two groups of paradoxes more briefly by saying that both the paradoxes of continuity and the paradoxes of universals are paradoxes of separation and inherence.

That Plato did not fail to notice this similarity appears from the fact that, in his *Parmenides* he discusses a series of problems which are of common interest to Eleatic and to his own philosophy. It seems that Plato did not accept the theory of Eudoxus,[2] and therefore it is understandable that he proposed an alternative solution of the paradoxes of continuity. These paradoxes show that we cannot consider the point as being the *element* of a line ; but the fact that a point, by its movement, can generate a line proves that nevertheless the point

[1] B. Bolzano, *Wissenschaftslehre,* Sulzbach, 1837, vol. 2, p. 15

[2] Plato, *Phaedo* 100 E. Epicharmus' anticipation (13 B 2) seems to me another reason to suspect the authenticity of the philosophical texts ascribed to him.

is the *principle* of a line.[1] This theory avoids the paradoxes of continuity, but it fails to afford a satisfactory explanation of measurement, whereas both the atomistic conception of the continuum and Eudoxus' theory of proportions did provide such an explanation. Plato tried to fill this gap by adopting, in addition to the point, a second principle, called the great-and-small, and specified into the long-and-short, the broad-and-narrow, etc. This principle was intended in particular to account for the possibility of measurement and thereby to solve a problem which had frequently occupied Plato.[2] Then Plato applied the same point of view in a solution of the Problem of Universals. Absolute entities are constituted by the co-operation of two principles : the One (corresponding to the point) and the Indefinite Duality (corresponding to the great-and-small). So absolute entities or ideas turn out to be related to geometrical magnitudes ; but they are not continuous, but discrete magnitudes, that is, *numbers*.

Now this reconstruction of the background of Plato's theory of ideal numbers, established by combining scattered material from different sources, is entirely corroborated by what is known about the theory itself. Until recently, the theory of ideal numbers was known exclusively from extensive fragments of Aristotle's account of Plato's lecture.[3] The recent discovery of extensive fragments of Plato's lecture *On the Supreme Good*[4] constitutes, therefore, a considerable progress in the study of Plato's philosophy. I shall try to give a summary of the main contents of Plato's lecture.

In Plato's opinion, the method of philosophy is similar to that of the grammarian. As language consists of words, words of syllables, syllables of simple sounds, these latter constitute the natural starting-point of the grammarian's enquiry. Similarly, the philosopher who investigates the universe must, to begin with, determine the elements into which the universe can be analysed. These elements or principles must be not only beyond sense perception, but also incorporeal. This is not meant to imply that any imperceptible and incorporeal entities which are found must necessarily be principles. For instance,

[1] For the terms 'element' and 'principle' cf. Aristotle, *Metaphysics Δ* 1–3 ; for Aristotle's comments on Plato's conception of the continuum, *Metaphysics*, A 9, 992 a 10 ff.

[2] Plato, *Protagoras* 356–7, *Republic* 524 C

[3] L. Robin, *La théorie platonicienne des idées et des nombres d'après Aristote*, Paris, 1908

[4] P. Wilpert, 'Neue Fragmente aus *Περὶ Τἀγαθοῦ*', *Hermes*, 1941, **76**. The most important fragment is Sextus Empiricus, *Adversus mathematicos* X, 248-80

neither ideas nor solid forms can be considered as principles, as both ideas and solid forms presuppose number. In fact, every idea in itself is one, but with regard to other ideas it is many ; consequently, as ideas participate in number, number must be more fundamental. But again this does not imply that numbers are to be considered as principles. For there is but one two, one three, etc. ; so every number participates in the One, which apparently is a principle of number and hence of all things. In addition to the One, Plato then postulates the Indefinite Duality as a second principle.

Now Plato has to show, conversely, that all things can indeed be deduced from these two principles. For this purpose, he introduces a series of distinctions.[1] All terms are divided into *absolutes* (for instance : horse) and *relatives* ; relatives again are divided into *opposites* (for instance : healthy-unhealthy, equal-unequal), and *relatives proper* (for instance : great-small, left-right). Each term in a pair of opposites is generated by the destruction of the other one, whereas the terms in relatives proper are always generated and destroyed together. Moreover, in each pair of opposites one term is definite, that is, does not allow of more and less, while the other is indefinite, that is, it does allow of more and less. You must either be healthy or not, but you can be more or less unhealthy. In relatives proper, both terms are indefinite. Now the absolute terms and the definite term in each pair of opposites are united in the class of the *definite* ; the indefinite term in each pair of opposites and both terms in relatives proper are united in the class of the *indefinite*. As all terms falling in one and the same class must have a common property, it follows from the Principle of the Idea that, for each of the two classes, there must be a corresponding absolute entity or principle. The absolute entity corresponding to the class of the definite will be the One, the absolute entity corresponding to the class of the Indefinite will be the Indefinite Duality. So from these two absolute entities or principles, all things must be generated. This generation of the world from the principles has also been described by Plato in his lecture, but unfortunately this part of Sextus' report is extremely obscure, no doubt on account of the obscurity of Plato's lecture itself.

The historical influence of the conceptions developed in Plato's lecture must have been considerable. Plotinus' doctrine of emanation, for instance, seems to be one of the attempts to develop Plato's sketch into a coherent and complete picture of reality. We shall see that

[1] Cf. Simplicius, *In Aristotelis physicorum libros commentaria* 247, 30–248, 15

Aristotle's philosophy is also strongly influenced by Plato's conceptions as developed in the lecture *On the Supreme Good.*

7 *Plato and Aristotle*

Usually a strong opposition is supposed to exist between the philosophy of Plato and that of Aristotle, and this has given rise to a dispute as to which of the two was right and which the more profound thinker. This outlook on the situation leads sometimes to rather eccentric conceptions such as, for instance, the opinion, defended by some adherents of the Marburg School, that Aristotle never understood Plato's doctrines. But, in recent years, scholars have realised more and more the close connections between Plato's thought and Aristotle's.[1]

It is true that Aristotle's works contain numerous objections to Plato's doctrines. But the polemical character of such critical observations should not be overestimated. Aristotle's aim was, most manifestly, to develop philosophy beyond the positions reached by Plato, starting from the final results of Plato's thought which in themselves show already considerable divergences with regard to the doctrines defended in most of the dialogues. A similar programme was adopted by those followers of Plato, especially by Speusippus and Xenocrates, his first successors, who remained faithful to the Academy; but these thinkers, lacking Aristotle's originality and feeling of independence, observed much more rigidly the teaching of the master. This situation forced Aristotle again and again to clarify his position with regard to the related views of Plato and the Academy with which, no doubt, his own opinions were often confused, the more so, as he had been himself for many years one of the most distinguished members of the Academy.

In fact, the ties between Plato's and Aristotle's views are numerous. Aristotle's theory of categories, for instance, derives directly from Plato's classification of terms, described above. The first book of the *Metaphysics* consists for a considerable part of a discussion of Plato's lecture *On the Supreme Good.* The differences of opinion are often

[1] It will be sufficient to mention the names of O. Becker, W. Jaeger, W. D. Ross, and J. Stenzel. For the connections between Plato's and Aristotle's views on mathematics, cf. K. Reidemeister, *Das exakte Denken der Griechen*, Hamburg, 1949, who rightly observes that, primarily, Plato was interested in pure, Aristotle in applied, mathematics.

overstressed and we can scarcely expect to obtain from Aristotle's comments a fair picture of Plato's views with which, moreover, the reader is apparently supposed to be already completely familiar. The general programme set forth in Plato's lecture, the search for the principles and for the manner in which the things generate from them, is taken over by Aristotle.

The following is another interesting example of the connections between Plato's and Aristotle's philosophy. Plato's endeavour to clarify the generation of things from the principles implies an important divergence to an earlier stage in the development of his philosophy, when he adopted Parmenides' thesis of the impossibility of change and movement, and a return to the outlook of the Ionian 'physiologists.' The arguments given for the acceptance of Parmenides' thesis are of a logical nature (though the hidden motives may have been of a quite different kind). For that reason the logical problems arising from the acceptance of the reality of change and movement were studied with remarkable thoroughness by Aristotle. His solution consisted in the construction of a logic of modalities, which was later revised by Theophrastus and Eudemus.[1]

Aristotle's theory of science has already been discussed sufficiently in my earlier article. I may therefore confine myself to observing that the evidence postulate is another consequence of the Postulate of the Absolute. This appears at once when we suppose $F(x, y)$ respectively to be the phrase : the terms x can be defined by means of the terms y, or the phrase : the sentences x can be derived from the sentences y.

8 *The Theory of Abstraction*

Scholz[2] has rightly observed that the traditional distinction between three levels of abstraction : the physical, the mathematical, and the ontological level, is, in this form, not found in Aristotle. It has originated from a fusion of two distinct theories of Aristotle's, the theory of abstraction and the theory of induction. The theory of induction is intended to explain our knowledge of principles ; it has already been briefly discussed in one of my earlier articles. The scope of the theory of abstraction is much more restricted ; this theory

[1] I. M. Bocheński, *La logique de Théophraste*, Fribourg, 1947

[2] H. Scholz–H. Schweitzer, *Die sogenannten Definitionen durch Abstraktion*, Leipzig, 1935

is intended to elucidate the ontological status of mathematical entities (numbers and geometrical figures) and forms part of Aristotle's criticism of Plato's theory of ideas.

Nevertheless, we can find in Aristotle some germs of the later theory of the levels of abstraction. In particular, I want to submit a few comments on an observation made in his explanation of the term *ποῖον* : quality is, 'in general, that which is inherent in the essence, apart from quantity ; for the essence of a thing is that which it is once.'[1] The usual interpretation of this text can be summed up as follows : (i) When we abstract from the specific qualities of the things, and retain only their mere individuality, then we obtain a mathematical number, that is, a multitude of unities which are only numerically distinct. (ii) When we abstract from the multitude of the individual representatives of a species, then we grasp the essence of this species which, as a unity of all specific qualities, is present in any of its specimens.

In order to connect a more definite picture with these somewhat vague statements, we consider a logical system known as the first-order calculus of monadic predicates with identity.[2] In this system, the sentence : *there are exactly three red roses*, can be expressed as follows :

$$(\mathrm{E}x_1)\ (\mathrm{E}x_2)\ (\mathrm{E}x_3)\ [a(x_1)\ \&\ a(x_2)\ \&\ a(x_3)\ \&\ b(x_1)\ \&\ b(x_2)\ \& \\ b(x_3)\ \&\,.\, x_1 \neq x_2 \,.\,\&\,.\, x_2 \neq x_3 \,.\,\&\,.\, x_3 \neq x_1 \,.\,\& \\ (x_4)\{[a(x_4)\ \&\ b(x_4)] \rightarrow [x_4 = x_1 \,.\, \mathrm{v} \,.\, x_4 = x_2 \,.\, \mathrm{v} \,.\, x_4 = x_3]\}] \qquad \text{(I)}$$

if a is taken to stand for *red*, and b for *rose*.

(i) Now we cancel the atoms $a(x_1)$, $a(x_2)$, . . . , $b(x_1)$, . . . which enabled us to express specific qualities. Then all that can be said is expressed by

$$(\mathrm{E}x_1)(\mathrm{E}x_2)(\mathrm{E}x_3)[x_1 \neq x_2 \,.\,\&\,.\, x_2 \neq x_3 \,.\,\&\,.\, x_3 \neq x_1] \qquad \text{(II)}$$

(ii) We can also, conversely, cancel the atoms $x_1 = x_1$, $x_1 = x_2$, . . . , $x_2 = x_1$, . . . , $x_1 \neq x_1$, $x_1 \neq x_2$, . . . , $x_2 \neq x_1$, . . . , which enabled us to express numerical identity and numerical distinctness. Then all that can be said is expressed by

$$(\mathrm{E}x_1)\ (\mathrm{E}x_2)\ (\mathrm{E}x_3)\ [a(x_1)\ \&\ a(x_2)\ \&\ a(x_3)\ \&\ b(x_1)\ \&\ b(x_2)\ \&\ b(x_3)] \qquad \text{(III)}$$

which is logically equivalent to

$$(\mathrm{E}x)[a(x)\ \&\ b(x)] \qquad \text{(IV)}$$

[1] Aristotle, *Metaphysics Δ* 14, 1020 a 35. For more details, see my forthcoming article, 'Logica en ontologie,' in *Alg. Ned. Tijdschr. v. Wijsbeg.*

[2] D. Hilbert–P. Bernays, *Grundlagen der Mathematik*, Berlin, 1934, vol. I

It will be clear that the operation, described under (i), is analogous to Aristotle's mathematical abstraction, whereas the operation, described under (ii), is analogous to his ontological abstraction.

It would carry us too far to continue this analysis and to apply it in a detailed criticism of the theory of abstraction. But it is worth while observing that ontological abstraction transforms sentence (I) into sentence (IV), which belongs to one of the four types which are discussed in Aristotle's syllogistics.[1] In general, a sentence of the first-order calculus of monadic predicates with identity will be transformed into a disjunction of conjunctions of sentences of a somewhat more general type. It seems to follow that there might indeed be some connection between Aristotle's views on abstraction and on the ontological status of universals and his syllogistics; but this is a matter which awaits further investigation.

9 *Reactions and Developments*

The following summary of reactions and developments provoked by the doctrines of Plato and Aristotle discussed above is given mainly for two reasons. First, these reactions and developments seem to confirm the somewhat daring interpretations which I have ventured to submit; secondly, they will, at least in part, provide interesting matter for further research. In my earlier articles I have already explained the intentions of the Megaric philosophers in stating the liar and larvatus paradoxes. I want to give some evidence in support of my explanation. That the liar paradox was indeed directed against Plato and Aristotle, is proved by the fact that Aristotle attempted to parry this attack, though his defence was very weak.[2] Our interpretation of the larvatus paradox as an argument against Aristotle's evidence postulate, plausible in itself, is corroborated by the wording of the dialogue; not content with refuting his opponent, the interrogator tries to make a fool of him by throwing back his 'of course'

[1] J. Łukasiewicz, *Aristotle's Syllogistic*, 1951, Oxford

[2] Aristotle, *De Sophisticis*, *Elenchis* 24, 25. The following views on Plato's relations with the Megarics are similar to those defended by Burnet. It seems to me that the rejection of these views by F. M. Cornford, *Plato and Parmenides*, 1939, London, p. 101, derives from the conventional underestimation of the Megaric School, which is reflected by the author's statement: 'The followers of Euclides soon gained a reputation for eristic, and they seem to have contributed nothing more important than some paradoxes which still provide logicians with amusement.'

(δηλαδή) as 'clearly' (δῆλος). It is well known that Aristotle frequently uses these words when he makes an appeal to self-evidence.

One question still remains to be considered: what caused the School of Megara to attack Plato and Aristotle so obstinately? I venture to give a tentative answer to this question. Originally Plato had accepted the Eleatic thesis of the impossibility of change and movement; he could then be considered, to a certain extent, as an ally of the School of Megara. But when, later, he dropped this thesis and was followed in this by Aristotle and perhaps also by other members of the Academy, this apostasy (publicly announced in the lecture *On the Supreme Good*, a subject in which the Megarics were especially interested but which was treated by Plato in a manner that could hardly be expected to satisfy them, as in addition to the One the Indefinite Duality was introduced as an independent principle, contrary to the strict monism which was a distinctive feature in Megaric philosophy and which, even in its detailed development, strongly recalls Spinoza's system) implied for them, not only the rupture with a distinguished and influential confederate, but also a considerable loss of face. Many scholars and men of science, who so far had followed indifferently in the lines of the Schools of Megara and Athens, may then have given their preference to the School of Athens. Such an event might well have caused considerable resentment among those who remained faithful to the School of Megara.

While the reactions in the School of Megara were mainly negative, Stoic philosophy, originating from remnants of some of the minor Socratic Schools but gradually taking over much of the prestige and influence of the Academy and the Lyceum, in several fields greatly improved upon the results of Plato and Aristotle. Łukasiewicz [1] rediscovered Stoic logic, previously underestimated and unduly neglected by historians of philosophy. It is understandable that his pioneer work is in need of revision in several respects. According to Łukasiewicz, the Stoics defended a linguistic interpretation of logic. This seems not to be correct. The Stoics made a clear and apposite distinction between (i) the *sound* of a word, (ii) its *meaning*, (iii) the *external object* which it denotes, and (iv) the *subjective image* connected

[1] J. Łukasiewicz, 'Zur Geschichte der Aussagenlogik,' *Erkenntnis*, 1935, 5. Cf. I. M. Bocheński, *Ancient Formal Logic*, Amsterdam, 1951, who quotes important unpublished work by Benson Mates. For the later development of ancient logic, cf. K. Dürr, *The Propositional Logic of Boethius*, Amsterdam, 1951.

with it.[1] Logic only deals with meanings. This doctrine, which strongly recalls modern discussions on *sense* and *denotation* of words and symbols,[2] enabled the Stoics to adopt, in their logical studies, a formalistic point of view and to ignore complications of a linguistic or psychological nature which they rightly considered as irrelevant in this connection; the usual reproach that they mixed up logic and grammar is, therefore, utterly devoid of justification and can be made only by scholars who do not sufficiently distinguish between logic and psychology, or who are unaware of the importance of sentential logic or of its independence with regard to predicate logic.

The difference of view, which Łukasiewicz supposed to exist between the Stoics and Aristotle with regard to the fundamental conceptions of semiotics, seems to me to be entirely absent. In point of fact, neither Aristotle's own words:[3] 'Proof does not refer to the external word, but to the word in the soul, for this is also the case with syllogism?' nor Alexander's comment:[4] 'Not in the words has the syllogism its being, but in the meanings,' contradict the conception of the Stoics. It appears rather that the Stoics refined Aristotle's somewhat indefinite doctrine by dividing his 'word in the soul' into the word-meaning and the corresponding subjective image, and that later, Alexander, in his commentary on Aristotle, took over the distinction made by the Stoics.

Łukasiewicz, discussing the Stoic definitions of the copulative and the hypothetical sentence, which enabled them to develop sentential logic on the same lines as it is usually done in modern logic, considered the corresponding definition of the disjunctive sentence to be lost. This last definition can be found in an interesting passage in Aulus Gellius.[5]

While Aristotle was content with giving an off-hand solution of the liar paradox, the Stoics did shrink from analysing it quite thoroughly. Rüstow, in a valuable study on the history of the paradox,[6] conjectured, rightly in my opinion, that Chrysippus' solution was preserved in the text:[7] 'they [who state the liar paradox] completely

[1] Sextus Empiricus, *Adversus mathematicos*, 8, 11

[2] R. Carnap, *Meaning and Necessity*, Chicago, Ill., 1947

[3] Aristotle, *Analytica priora* A 10, 76 b 24

[4] Alexander Aphrodisiensis, *In Aristotelis analyticorum priorum librum I commentarium* 372, 29, quoted by Łukasiewicz, loc. cit.

[5] Aulus Gellius, *Noctes Atticae* XVI, 12–14

[6] A. Rüstow, *Der Lügner* (Thesis, Erlangen), Leipzig, 1910

[7] Papyrus Herculanensis 307, col. 9, 12ff., 23ff.

stray from word meaning; they only produce sounds, but they don't express anything.' This solution, which reminds one strongly of modern discussions on the subject, was not accepted by the majority of his followers. The solution which the majority of Stoic philosophers preferred to Chrysippus' was considered by Rüstow as lost. I think it may be preserved in the well-known Stoic definition of truth and falsehood:[1] 'True is that which happens to be and is opposite to something, and false that which does not happen to be and is opposite to something.' The statement, ascribed to the liar, cannot be supposed to contradict anything, and therefore, on account of the afore-mentioned definition, it can be neither true nor false. It follows then, from the Stoic definition of an assertion,[2] that the liar's statement cannot be an assertion. So it does not constitute any difficulty for logic. The same solution, substantially, was defended much later by Bolzano.[3]

10 *Conclusion*

In historical research on Greek mathematics, astronomy, physics, and biology it is customary to approach the problems which occupied the Greeks from the point of view of modern science. A similar method has been introduced in the study of Leibniz' logic and metaphysics by Russell and Couturat, and its application proved extremely fruitful in the work on Greek formal logic initiated by Łukasiewicz. The preceding pages have shown, it is hoped, that also in the study of Greek philosophy we may obtain new and more satisfactory results by looking at its problems from the point of modern philosophy of science, liberating ourselves from the influence of the philosophy of Plato and Aristotle, and of traditional speculative philosophy in general, regardless of the fact that speculative philosophy has, more than the fields of science mentioned above, been influenced by motives of a non-scientific nature.

Though Greek philosophy did not, like Greek mathematics and Greek astronomy, leave behind contributions to human knowledge which will always be acknowledged integral parts of science, it originated no less from the great Hellenic effort to provide a rational treatment of the problems which experience proposes to mankind.

[1] Sextus Empiricus, *Adversus mathematicos* VIII, 10

[2] Diogenes Laërtius, *De vitis*, etc., VII, 65

[3] B. Bolzano, *Wissenschaftslehre*, Sulzbach, 1837, vol. 2, p. 80

We should be ready to recognise and admire it as a serious endeavour to reach a level of abstract thought which has only in our time become definitely attainable. So it seems just to consider Plato and Aristotle as the predecessors of men such as Cantor, Frege, and Russell.

Only in our time has a point been reached from which traditional philosophy can be surveyed in its true internal structure and its real historical context. The field of Plato's and Aristotle's speculations, the starting-point of philosophical tradition, turns out to constitute only a very small part of the vast domain covered by modern abstract thought. If we start from the positions secured by modern science, we can approach this field from many different angles. In this article, we could only discuss a few examples of the results obtainable in this manner.[1]

[1] The author is indebted to Dr A. C. Crombie, Professor W. K. C. Guthrie, and Mr David Furley for helpful criticism. To his regret, he has not been able to consult Mr J. E. Raven's *Pythagoreans and Eleatics* (Cambridge, 1948) or Sir David Ross' *Plato's Theory of Ideas* (Oxford, 1951).

Department of Philosophy
University of California

Time of Psychology and of Physics

Dr Dobbs, in his recent articles under the above title[1] discusses the significance of quantum theory considerations on the specious present. The time quantum has been defined in a number of ways, based on the idea that no interval of time can be meaningful if it is less than that required for light to traverse the smallest sharply-defined interval of space. Unless the further elucidation of Philpott's[2] remarkable work should, on further analysis, provide evidence to the contrary, it is hard to see what the time quantum can have to do with time intervals of the order of a few seconds. I do not fully appreciate Dr Dobbs' reasons for believing that the spread of 'compresence' in the realm of sub-atomic physics increases by so many orders of magnitude 'when a highly resonant-coherent structure, such as a human brain is concerned.'[3] I cannot say that I find the suggestions given on pages 188 and following very convincing, but perhaps Philpott's work will lend experimental evidence to the case for linking quantum considerations to time intervals of an order of magnitude such that they can be consciously appreciated.

Philpott has found that the periodicity of fluctuation in human output can be reduced to a basic time unit $\sim 10^{-23}$ sec., i.e. the same order of magnitude as the time quantum. This is, I believe, the only direct link between the time quantum and times of the order of the specious present.

Even 'compresence' includes, however, only comparatively brief stretches of first-dimensional time. Dr Dobbs mentions the work of Price on Precognition: how do the findings of Rhine[4] and others (so ably discussed by Thouless[5] which suggest at least a statistical precognition over periods of weeks or months, fit into the picture?

Dr Dobbs does not give a complete account of the historical development of the concept of multidimensional time. It seems likely

[1] H. A. C. Dobbs, this *Journal*, 1951, **2**, 122, 177

[2] S. J. F. Philpott, *Brit. J. Psychol.*, 1950, **40**, 137

[3] Dobbs, *ibid.* pp. 135-6

[4] J. B. Rhine, *The Reach of the Mind*, London, 1948

[5] R. H. Thouless, *Roy. Inst. Even. Meeting Rep.*, 1st Jan., 1950

that Dunne, whom he does not mention, but whose work in this field is well known, must have been much influenced by Bergson. Bergson [1] emphasised that to *perceive a line as a line* one must think in terms of a three-dimensional space. (Note that he did not say that the *use* of an n-dimensional continuum necessitated the postulate of an $n + 1$ dimensional continuum.) Dunne [2] 'spatialised' time, attempting to perceive it as a line, and was obliged to postulate a second time dimension in which to describe the rate of passing of his linear time. This led to an infinite regress of times [3] but Dunne concluded [4] that there is no practical object to be achieved in considering more than two terms of the series. The specious present is explained by asserting that the focus of attention in first-dimensional time is surrounded by a narrow fringe of attention in the second dimension.[5]

The infinite regress, as Dunne seems partly to have realised, comes about as a result of a confusion of the observer with the observed. A picture of the entire Universe must include the artist in it but, as I have pointed out elsewhere,[6] how can he include himself, except by the trick of a mirror? How else can he depict his own eyes? (Dunne [7] substitutes a passing yokel as a 'stand-in'—but this really will not do!) Using a mirror puts the artist in the wrong time sequence with respect to the rest of the picture, since light takes a finite time to reach the mirror and to return. (For the complete argument see my *Measurements of Mind and Matter*, p. 99.) The dilemma is seen to be insoluble.

Certainly if we spatialise time, then we must accept at least two dimensions for it. The question is, how far can we follow Dr Dobbs in treating time as two separate 'aspects,' the intensive and the extensive?

No one denies the usefulness of being able to include time within the geometry of space co-ordinates through the operation of i; but this, contrary to the belief of some early relativists, does not mean that 'henceforth space and time in themselves vanish to shadows and only a union of the two exists in its own right.' On the contrary,

[1] H. Bergson (transl. F. L. Pogson), *Time and Free Will*, London, 1910, p. 103

[2] J. W. Dunne, *An Experiment with Time*, London, 1946; *The Serial Universe*, London, 1945

[3] Dunne, *An Experiment with Time*, p. 110

[4] *Ibid.* p. 187

[5] *Ibid.* p. 161

[6] G. W. Scott Blair, *Measurements of Mind and Matter*, London, 1950; this *Journal*, 1950, **1**, 230

[7] *The Serial Universe*, p. 30

as Johnson [1] puts it, 'To measure length, you must say what you mean by two points at the same time. . . . To measure a time, you need only say what you mean by two events at the same place, namely in your individual consciousness.' Alternatively, an essential difference between time and space lies in the fact that we cannot be in two different places at the same time but are often in the same place at two different times.

The spatialising of time leads, logically enough, to an Identity Theory such as that of Blamey Stevens [2] who regards time and space (and, incidentally also inertia) as 'identical' in the sense that our perception of them is distinct only because the intervals which we perceive as times are so very large compared with those which we perceive as distances. 'By one set of perceptions we conceive of a certain thing as measuring a metre, and by another set of perceptions we conceive another thing as measuring a second. The last is 300,000,000 times as great as the first' (p. 17). But I cannot see that this gets us very much further.

I cannot agree with Dr Dobbs that Milne's τ and t times have anything to do with the dimensional problem, nor even that there is an 'obvious similarity.' These time-scales refer to two isoperiodic systems within the same dimension and are essentially comparable, in the spatial field, to a standard meter and a standard yard, except of course that the differences between the two scales is so very small.

Finally, a word about intermediacy and the quotation (p. 178) from Dirac's *Quantum Mechanics*. Prigogine [3] relates his thermodynamic time (τ) to Newtonian time (t) by an equation which, for certain equilibrium conditions, he writes as :

$$\tau = t_0 + t_0 \log (t/t_0)$$

We must be careful of nomenclature here. The equation is apparently identical with Milne's, yet Milne used τ for dynamic or Newtonian time and t for kinematic or 'radioactive time' as Prigogine calls it. The 'cross-over' is due, I believe, to a different definition of the epoch at which the time scales are to coincide.

Prigogine claims that his thermodynamic time is non-metric and cannot be spatialised, but I can see no reason why this should be so.

[1] M. Johnson, *Time, Knowledge and the Nebulae*, London, 1945, p. 116

[2] B. Stevens, *The Identity Theory*, London, 1936

[3] I. Prigogine, *Étude Thermodynamique des Phénomènes Irréversibles*, Paris and Liége, 1947

This is quite unlike the 'becoming' aspect of time. His equation corresponds, of course, to du Noüy's, for 'biological time.'[1] Before equilibrium is reached, there is a disposable parameter, depending on the rate of increase of entropy, which defines the different logarithmic time scales and in general, as von Bertalanffy[2] points out, the relation between two such exponential equations can be expressed as a power law.

Hence, if a particle is moving at constant velocity with reference to scale τ_1 we can write the equation for its motion in terms of scale τ_2, using fractional differentials, as follows :

$$\frac{dl}{d\tau_1} = \frac{d^n l}{d\tau_2^n} = \text{const.}$$

(Where n is the fraction which defines the relationship between the two time scales.)

The expression $\frac{d^n l}{d\tau_2^n}$ is intermediate, in τ_2-time, between a length and a velocity.

These fractional differentials form an infinite group and I have shown[3] that an equation derived from them by integration adequately describes the behaviour under stress of many materials whose properties lie intermediate between those of liquids and solids. The fractional differential approach also greatly simplifies the description of certain psychophysical data[4] and thus serves as a useful link between the time of psychology and physics.

I have also attempted[5] a comparison between this Gestalt approach ('Principle of Intermediacy') and the use of Factor Analysis, referred to by Dr Dobbs at the beginning of his second paper ; but this question requires much further study.

G. W. Scott Blair

[1] Lecomte du Noüy, *Biological Time*, London, 1936

[2] L. von Bertalanffy, this *Journal*, 1949, **1**, 134 ; *Nature*, 1949, **163**, 156

[3] G. W. Scott Blair, B. C. Veinoglou, and J. E. Caffyn, *Proc. Roy. Soc.*, 1947, **189A**, 69 ; G. W. Scott Blair and J. E. Caffyn, *Phil. Mag.*, 1949, **40**, 80

[4] G. W. Scott Blair and F. M. V. Coppen, *Amer. J. Psychol.*, 1942, **55**, 215 ; 1943, **56**, 234 ; R. Harper, *Amer. J. Psychol*, 1947, **60**, 554

[5] *Measurements of Mind and Matter*

Concepts out of Context

THERE are several points in Mr Pirie's article under this title that many biolcgists will wish to question.

In his first two pages Mr Pirie is, I think, unlucky. He says that we often suffer from illusions in dealing with both our own and neighbouring sciences, and on the next page he falls into what very many of the biologists who are concerned with the classification of organisms would say is just such an illusion. He says that 'the evolutionary origin of species is bound to give intermediate types and there may be a transient continuity between adjacent species,' so that the species is necessarily a vague concept incapable of accurate definition. That certainly was Darwin's view; it was the general view in the latter part of the nineteenth century and indeed until twenty or thirty years ago. But opinion is now different, and I think that the majority of systematists would say that the species, at least in zoology, is an objective fact of nature that can be defined as clearly as other biological concepts. Mayr,[1] for instance, whose book is accepted as authoritative by many, shortly defines species as 'groups of actually or potentially interbreeding populations, which are reproductively isolated from other groups.' This new outlook on the species problem—and on other problems of systematics—has been mainly due to realisation of the value of ecology in systematics. The older view neglected the facts that on any theory of evolution interbreeding is necessary for the production of intermediate forms, and that interbreeding does not normally occur in nature between forms that have reached the species level of differentiation, though occasional hybrids may be found. Perhaps ecology is of as great value in the systematics of bacteria—and of viruses, if we were to believe them to be organisms—as in the systematics of the animals.

Then, again, there is Mr Pirie's assertion that life cannot de defined. A concept may be defined though we cannot draw an exact line around the objects to which it applies. The concepts hard and soft are real and definable, but we cannot draw a line exactly delimiting what is hard and what is soft; the value of the concepts lies in that they allow us to say that one thing is harder or softer than another. So, the living organism possesses characters which, occurring together, distinguish it from the non-living and allow us to define life, that is, to point

[1] E. Mayr, *Systematics and the Origin of Species*, New York, 1942, p. 120

to some things which are alive and others which are not alive. It may be that there are objects that possess some but not all these characters, and therefore cannot be definitely placed in either of the categories living or non-living.

Thirdly, in discussing efficiency, Mr Pirie says, 'a theoretical advantage may well come up against the principle *de minimis non curat lex*.' But he does not consider the results of Fisher [1] and Sewell Wright,[2] who have shown that an extremely minute advantage or disadvantage—$1/10^5$ or less in large natural populations—will be effectively controlled by selection. If these results are accepted, the difficulty many biologists feel is to understand how *any* characters in these populations can so little affect the efficiency of the organism as to escape the control of selection.

G. S. CARTER

[1] R. A. Fisher, *The Genetical Theory of Natural Selection*, Oxford, 1930
[2] S. Wright, *Genetics*, 1931, **16**, 97

REVIEWS

The Alchemist in Life, Literature and Art, John Read, Thomas Nelson and Sons Ltd., Edinburgh, 1947. Pp. xii + 100. 10s. 6d.

ALCHEMY, like its subject, is a very Proteus, and every man who seeks to grasp it sees it under a different form. Professor John Read, who has written extensively on this elusive subject, finds his delight in the face that alchemy shows to the world, the fascinating circumstances and the vivid symbolism of the Art, the oddities of those that practised it. The present work is divided into three sections. The first, on Alchemy and Alchemists, gives a general description of alchemical theory and practice. The second is devoted to the alchemist in literature, and the third to the alchemist in art.

The section on alchemical theory is slight. True it is that each of us sees a different aspect of the art ; to the reviewer the central tenet of alchemy is the existence of the philosopher's mercury, the quintessence, the tingeing spirit—to give it only a few of its names, whereas the author regards the essence of alchemy as its views of the nature of the elements that underlie matter and especially the metals, views generally shared with the orthodox natural philosophers of the time. The whole of this section of the book describes, rather than attempts to explain, the theory and practice of alchemy.

The second section, on the Alchemist in Literature, is written with the charm that we have learned to expect from Professor Read. Two sections deal with Chaucer's *Canon's Yeoman's Tale* and Ben Jonson's *The Alchemist*, both of which will be familiar enough to most of us and especially to the lover of alchemy. The third section, however, deals with the less known Simon Forman—an amusing and disreputable character, rather more magician and astrologer than alchemist. There is a good deal of interesting matter about Forman among the Bodleian MSS. to which, however, Professor Read makes no reference ; conspicuous is a sprightly, though fragmentary, biography of Merlin.

The third section of the book on the Alchemist in Art is the longest and most interesting, being illustrated with a number of excellent plates. The author's theory of the alchemical meaning of Dürer's Melancolia is interesting and well argued ; the reviewer would be more convinced had he not heard so many other and ably argued accounts of its meaning. Against the alchemical interpretation of this picture may be set the fact that Dürer is well known to have had the deep and semi-mystical interest in mathematics of his age, but is not known to have concerned himself with alchemy ;

the theory that the picture expresses the mystery and power of mathematics seems, therefore, more tenable. The other pictures, many by David Teniers, receive interesting comment.

It cannot be said that this work adds to our knowledge of alchemy, nor perhaps was it intended to do so. Rather is it a pleasant and scholarly manuduction into alchemical by-ways, which every lover of the past, be he historian of science or not, will be happy to explore.

F. SHERWOOD TAYLOR

Science and Literary Criticism, Herbert Dingle, Thomas Nelson & Sons, Ltd., Edinburgh, 1949. Pp. 184. 7s. 6d.

WHEN a distinguished philosopher of science essays a contribution to the theory and practice of literary criticism, we are whole-heartedly prepared to applaud such versatile enterprise ; and, after reading Professor Dingle's book, we can still applaud the zeal and rigorous cerebration that he brings to a subject outside his bailiwick. At the same time, if we consider the book in itself, as we must, and ask whether it helps much to clarify and systematise the traditional aims and methods of criticism, we cannot—at least this reviewer cannot—say that it does.

Professor Dingle recognises the obstacles in the way of a scientific approach to works of art, the inevitable variability of individual response and the lack of objective and uniform criteria. He is acute also in discovering fallacies in the more or less scientific theories of some predecessors. These make a rather odd medley—Sainte-Beuve and Taine, R. G. Moulton and J. M. Robertson, and T. S. Eliot and I. A. Richards. Although the last two have demanded precision in thought and language, and although Professor Richards (whose later books are not touched) has aimed at a really scientific criticism, Professor Dingle finds even them guilty of meaningless pseudo-statement and subjectivity.

Psychological criticism, the relating of literature to the psychological character of the author, is outlawed, because this is a theory of human mentality and criticism and ' concerns the product only ' (pp. 36-7). ' But while the psychology of authorship is outside criticism, it is quite otherwise with the psychology of readership,' for ' Interpretation is a psychological process ; hence the psychology of interpretation is essential to criticism ' (p. 46), although we have already been told (p. 33) that ' it is impossible . . . to compare the feelings of one person with those of another.' Might not all this be called a chain of dogmatic and contradictory pseudo-statements ?

Notwithstanding dubious things in these early chapters, Professor Dingle, like most people, is more successful in criticising others' theories

than in developing his own. Science he defines (p. 17) as the establishment of relations between commonly accepted data, and he would carry this principle into literary criticism. The critic's data are the poems, and he will seek to construct their source, the poet, who ' will be an extremely generalised person,' the composite of ' qualities that are expressed in all the poems, regardless of their peculiarities ' (p. 78). Thus ' we try to explain the man by the work—or, rather, create an idea of a man who could produce the work.' But, if criticism concerns only the product, why, even as a prologue or prompter to criticism of individual poems, do we first seek to compose a generalised portrait of the author ? Is not the scientist somewhat misled by his analogy of scientific hypothesis ? This ' hypothetical poet,' moreover, ' is also the sole legitimate contribution of criticism to the individual psychology of the actual poet ' (p. 82) ; and yet, contrasted with Professor Dingle's previous rejection of psychology, it would seem to be a pretty large contribution.

It is in fact about the whole contribution of the three critical essays that form the second part of the book. These essays on Wordsworth, Swinburne, and Browning, though written earlier, are modestly offered as specimens of scientific criticism. Remembering that criticism must be based on ' all the poems,' we may sigh for the critic attempting to assess these voluminous poets, but we learn at once that the canon is to be reduced to ' that portion of it which the common poetic instinct of humanity recognises as inspired '—an unverifiable criterion of value which Dr Richards (pp. 54-5) was censured for using. In each essay Professor Dingle constructs, from the poetry, a generalised ' hypothetical poet ' and defines his central, if unconscious, impulses and philosophy. However, if we were not told, we might not have discerned the operation of a scientific method—and some such aim has been traditional in criticism. Apart from a few allusions, these essays might have been printed as obituary estimates in 1850, 1909, and 1889. Wordsworth seeks happiness in passive reverence for the Spirit of Nature ; Swinburne has a passion for the infinite ; Browning sees life as a set of problems in terms of solutions already apprehended. Surely, after all the scientific build-up, this is a little disappointing. We might, too, have expected some aesthetic and technical criticism, a department in which scientific precision might contribute something, but of that we get very little.

Throughout the essays Professor Dingle makes many suggestive general observations, though they hardly seem scientific. And in his emphasis on a general norm he can neglect or iron out significant ' peculiarities ' ; both he and his poets can become victims of the unifying formula. When, in keeping with his thesis about Wordsworth, he makes the unqualified statement that ' He entered into the phenomenon of the French revolution for its own sake—or rather for the sake of the delightful experiences it

engendered—without thought of the reform it was to effect' (pp. 122-3), he is either disregarding or disbelieving the poet's ample testimony. In keeping with the thesis about Browning, he says that 'the literary value of the utterance is proportionate to the conviction in the solution' (p. 161); is *Rabbi Ben Ezra*, then, a better poem than *The Bishop Orders his Tomb*? At any rate Professor Dingle convinces the reader that scientific criticism is impossible. It might be added that, while his thinking is active and provocative, his range of critical reading is very insular; though he breaks a lance with Messrs Eliot and Richards, he seems unaware of modern developments in criticism. But we may still wish there were more scientists like Professor Dingle in the world.

DOUGLAS BUSH

The Human Use of Human Beings, Norbert Wiener, Eyre & Spottiswoode (Publishers) Ltd., London, 1950. Pp. xii + 241. 18s.

THREE years ago Norbert Wiener published his *Cybernetics*: it was a serious discussion of the problems of communication and feedback and of their importance in the machine and the living organism. But it was a little 'Third Programme' in its style. Now, in his *The Human Use of Human Beings* we have the 'Home Service' version. The word Cybernetics has been relegated to the sub-title, and all the mathematics discreetly dropped. But the mixture is really the same as before. Thus, an increasingly large part of the physical activities of a working man can now more adequately be performed by a machine, and a large part of his mental activities can be simulated in much the same way. This means that our society is passing through its second Industrial Revolution; greater social changes are impending than our fathers could ever have suspected. Either we adapt ourselves or, like the great unwieldy dinosaurs, we shall perish. Voices of Rigidity, whether of Church or Communist State, are clamping down upon that vital spark which lies in us. 'The hour is very late, and the choice of good and evil knocks at our door.'

With much of this we should most of us agree. I have myself visited a factory where dangerous chemical reactions were taking place, but where the only workman was the engineer who came periodically to see that the recording instruments really were recording; and I can well believe that before long the motor car assembly line will be similarly controlled. I think I can accept the belief that 'a new war would almost certainly see the automatic age in full swing within less than five years.' It is high time that voices were sounded to underline the immensity of our problems, and the 'non-linear' character of the changes that we see taking place all round us.

Yet there are two things I feel I want to add by way of comment. The first is that though cybernetics may simulate much of the working of men's

minds, yet the human race is much more than an extremely efficient calculating machine, even than a calculating machine that can learn by adaptation. Wiener hints at this when he says that 'nothing less than the whole man is enough to constitute the scholar, the artist and the man of action.' But his medicine is directed only at one aspect of man's total nature. When he claims that 'to be alive is to participate in a continuous stream of influences,' he is correct, but his description is incomplete. Learning, and communication, will enable us to *do* things ; they will not necessarily enable us to *be*. And yet 'doing' and 'being' are both part of man's fulfilment. I cannot help thinking that the Christian doctrine of the Beatific Vision would be quite incomprehensible to Professor Wiener. The explanation is simple : it is almost obvious if we notice that the words 'self' and 'self-consciousness' do not appear in the index, though there are nine full pages of it. This means that the author is writing from a broad humanistic outlook. What he lacks, in common with many other humanists, is a clear picture of what constitutes a human being. It is all very well to say that 'the mechanical control of man cannot succeed unless we know man's built-in purposes, and why we want to control him.' But there is not the slightest hint of this 'know-why,' to supplement the 'know-how' with which the book abounds.

My second comment must be briefer. It is concerned with the philosophy of science which underlies this book. The book itself is written on the assumption that the world is something existing entirely apart from the observer, and which does possess the power to send out so many unit signals of information, whose nature and frequency have an objective validity. If Wiener were told, in Jeans' famous phrase, that 'the Universe has become more like a thought than a thing, a thought in the mind of a great mathematician,' he would be astonished and nonplussed at the first part, since the very concept of communication and feedback would be seen to be in need of more clear definition ; but he would not be surprised in any way whatever that the mind was that of a mathematician, for the whole of this entrancing and exhilarating book is a diatribe in favour of mathematics.

C. A. Coulson

Out of My Later Years, Albert Einstein, Thames and Hudson Ltd., London, 1950. Pp. viii + 282. 15s.

To most of his contemporaries Einstein is somewhat of a mystery. Thus he was one of the founders of modern physics ; yet, almost alone among the great theoretical physicists, he does not accept the conclusions of physics, and still continues searching for a mathematical scheme in which Nature will preserve the objectivity of which both quantum theory and recent

philosophical thinking have largely robbed it. Again, though a staunch Jew, he disbelieves in any sort of personal God ; and although in one of the papers quoted in this book (p. 114) he can assert that science has nothing to do with values, in another (p. 26) he tells us that ' science without religion is lame, religion without science is blind.' Those who might hope to distil from the 59 extracts reported in this book something of the real ' inner ' character of the man will find it no easy matter. At least they will see this : that under the slightly forbidding exterior, there lies a warm, liberal, friendly and, sometimes, passionate heart, and a sympathetic and humanitarian mind interested in almost everything except sport, and not afraid to speak out occasionally to utter statements which would be difficult to sustain. A man who can say that ' no purpose is so high that unworthy methods in achieving it can be justified in my eyes ' has already committed himself to so much that we shall suspect that his heart really rules his mind. This seems to be the only satisfactory interpretation of those puzzling inconsistencies I mentioned earlier. It is hardly fair to expect a collection of excerpts from broadcast addresses, public lectures, obituary notices, scientific polemicals and serious scientific work to tell us everything, especially when they cover a range of years from 1933 to 1950. Myself I find these excerpts, some of which are less than 20 lines long, too ' bitty ' for my liking. Nevertheless, as revealing the reactions of one of this century's great men, they serve a purpose—but that purpose is not scientific.

C. A. Coulson

The Growth of Scientific Ideas, William P. D. Wightman, Oliver and Boyd, Edinburgh and London, 1950. Pp. xiv + 496. 25s.

The author, who is Reader in the History and Philosophy of Science in the University of Aberdeen, modestly describes this book as ' a preparation for the study of the History of Science.' A knowledge of history, he points out, is necessary ' not to " liberalise " Science [the ambition of some recent educational reformer], but to understand it.' Making no claim to be writing another general history of science, Dr Wightman's aim is, by adopting the method of historical inquiry advocated by the late R. G. Collingwood, to display ' the development of scientific thought as illustrated by a few dominant ideas.' His method of doing so is ' first to set out the expressed views of each thinker—in his own words so far as space permits. In attempting an interpretation of each thinker, I shall try to answer the following questions : (*a*) What is he trying to do—that is, to what unspoken question is he trying to find an answer ? (*b*) Why has this question assumed a special importance for *him* ? (*c*) What is the nature of his answer ? (*d*) How far did it satisfy his contemporaries ? (*e*) How far does it satisfy

us—that is, to what extent was his answer a *real* answer.' Thus we '*relive* the ideas of the past in order that, by watching them grow, we may the better understand what scientific ideas really *are*' (p. 8). And Dr Wightman concludes, 'if the history of science teaches us anything it is that only to those who ask the right questions are any answers vouchsafed. Not even to them are these answers the end of the quest, but only an enlargement and a refining of the question. If we look back to see whence we have come, we may the better set the course on which we are to go' (pp. 474-75).

The book is divided into two unequal parts, the first and longer being entitled 'Matter and Motion,' the second 'Nature and Life.' Part I begins with a brief account of the work of the Greek geometers and natural philosophers. The strategic step taken by the Greeks was to introduce the notion of deductive proof; the generality and rigour so achieved made Greek science something essentially different from the *ad hoc* techniques, many and varied as they were, developed in the ancient civilisations of the Near East. Next come three chapters taking 'the geometry of the heavens' from the work of Greek astronomers down to that of Galileo and Kepler. Five chapters follow on the development of seventeenth-century mechanics from Galileo (including an interesting analysis of his scientific method) to the Newtonian Revolution. These are succeeded by groups of chapters on mainly post-seventeenth-century physical problems: on colour and the nature of light, on the nature of matter and combustion (a chapter on Dalton's atomic theory gives an excellent account of the relation between the hypothesis and the experimental facts), on heat, magnetism and electricity, and on thermodynamics and electromagnetism since Maxwell. Part II, on the biological sciences, begins again with Greek work. There is an interesting discussion of the different biological implications of the atomist theory that all change is the rearrangement of persisting parts and of Aristotle's theory of matter and form. After a brief account of the bearing of the work of the Greek physiologists on the question 'What is life?' Dr Wightman passes rapidly through that of the Renaissance anatomists to the mechanistic researches and conceptions of Harvey, Borelli, Descartes and Hales. Chapters follow on early microscopy and its contribution to theories of generation, on the development of taxonomy, on pre-evolutionary morphology and the interpretation of comparative morphology by pre-Darwinian evolutionists, on the Darwinian Revolution, on the rise of nineteenth-century physiology and of Mendelian genetics.

Dr Wightman's exposition of these broad themes is on the whole well done. He writes clearly, keeps the main point of each chapter well in view, and shows how isolated problems have coalesced to form the major branches of science and how new ideas and techniques, for example those of seventeenth-century mechanics and of nineteenth-century evolutionary biology, have spread to other branches besides those in which they began,

often being transformed in the process. In this way the book as a whole has sustained interest. Moreover, the method of approach is illuminating. By trying to get at the question a scientist of the past was asking, Dr Wightman has been able to make the answers intelligible, remote as they often were from twentieth-century conceptions. This is a truly historical approach. For the reader it is also a valuable exercise in the comparative method. The book can be strongly recommended both to students of the history of science in the higher school forms and at the University, and to the general scientific and historical reader.

The main point of criticism I have to make of Dr Wightman's history is that it gives no evidence of any knowledge of the published results of the last half-century's research into medieval Arab and Western science. This is a fault almost universal in text-books in English on the history of science, but that hardly excuses it. No doubt every Bantu questioned about Newton's theory of gravitation would show an equal ignorance, but few English-speaking scholars would wish the comparison to be pressed. The result in Dr Wightman's book is that the period from Ptolemy to Copernicus, from Galen to Vesalius, appears as a void inadequately relieved by a few passing references to Arab and Western optics and medicine and to Albertus Magnus, Buridan and Cusa, and brief discussions of medieval alchemy, of Petrus Peregrinus' work on the magnet, and of pre-Vesalian anatomy. This is not the place to give a bibliography of recent work on Arab and Western science in the middle ages: interested readers should consult Sarton's *Introduction to the History of Science* and the 'Critical Bibliographies' published regularly in *Isis*. It is especially unfortunate that Dr Wightman has taken no account of the work of Pierre Duhem, whose last volume appeared in 1917. Duhem's historical approach, with its strong emphasis on the questions behind the work done by past scientists and on their methods of answering them and of testing the answers, was similar to that adopted by Dr Wightman. Use of Duhem's account of medieval work on method and on particular scientific problems, together with the results of more recent scholarship, would not only have filled in Dr Wightman's blank period but also have put his account of sixteenth-and seventeenth-century science in truer perspective.

It may be useful to enumerate the following mistakes of commission and omission, some of them the result of Dr Wightman's neglect of medieval thought: Most of the discoveries made in the Middle Ages remained in the Western scientific tradition, of very few is there evidence that they were 'isolated discoveries . . . forgotten again' (p. 44); it is quite untrue to say that the Arabs made 'little advance' on Greek science, and to say that in the medieval West 'it was considered heretical to pass' beyond Aristotle's words (or opinions) (p. 44); Copernicus was a secular canon, Bruno a Dominican friar, neither was a monk (p. 44); notable work on lenses was

done not only by the Arabs but by Western writers in the thirteenth and fourteenth centuries : the latter invented spectacles (p. 57) ; the statement, *sans phrase*, that the seventeenth century saw a ' shift of emphasis from preoccupation with *words* to the observation of *things* ' is quite misleading : the chief innovation of Galileo was not his use of observation and experiment, both of which had been well understood since the thirteenth century, but his use of mathematics, the point of much of his polemical writing being that his predecessors had been *too* empirical (pp. 61-2) ; Galileo's proof of the equation which we write $s = \frac{1}{2}at^2$ was identical with that given by the fourteenth-century mathematician, Nicole Oresme (p. 64) ; the title of Cornford's book should read *The Laws of Motion in the Ancient World* (p. 69) ; the experiment described by van Helmont had been described two centuries earlier by Nicholas of Cusa (pp. 168-9) ; it is hardly true to say that Peregrinus made his experiments in order to construct an engine capable of perpetual motion, ' a wild-cat scheme . . . characteristic of an unscientific age ' : the bulk of his *Epistle* consists of a very sober account of his investigations on the magnet, the perpetual motion wheel being discussed at the end as one of ' the ingenious contrivances which depend on a knowledge of its natural workings,' others being instruments for determining azimuths (p. 207) ; the account of Buridan's and Cusa's dynamics is hardly serious (p. 271) ; by Newton's day action at a distance was a very old problem, much discussed in the fourteenth century (p. 302) ; ' ibn Siena ' should read ' ibn Sina ' (p. 335) ; Galen's writings were translated into Latin from both Greek and Arabic in the twelfth and thirteenth centuries, long before Linacre ; Western medicine and surgery did not decline but steadily improved after the early days of Salerno (p. 336) ; the ' reign of Authority ' said to have been closed with Harvey was more humanist than scholastic (p. 340) ; four centuries before Harvey, Albertus Magnus (who discovered the true insect egg) said ' generatio omnium animalium primo est ex ovis ' (p. 353) ; Giordano Bruno, whose condemnation Professor Thorndike, Dr Mercati and Mrs Singer have shown to have been for his theological heresies and not for his scientific or pseudo-scientific opinions, has been made to play many remarkable roles in the history of science, but perhaps none more bizarre than this : ' An explicit disavowal of the changelessness of the form of the Universe was liable to lead to martyrdom, as was the case with Giordano Bruno (1600)—perhaps the first modern pioneer of evolution ' ; the true nature of fossils was understood by some Greeks and by Avicenna, whose ideas were adopted and illustrated with interesting observations by Albertus Magnus in writings well known down to the seventeenth century (p. 396) ; eighteenth-century ideas on evolution are worthy of serious attention but are scarcely mentioned here (p. 397) ; Wallace's ideas on evolution and natural selection are grossly underestimated (p. 410) ; part of the modern theory of infection, including the notion of

infective 'seeds,' was developed by Arab and Western doctors at the time of the Black Death (p. 448); a theory of pangenesis similar to Charles Darwin's was discussed in 1745 by Maupertius, the first writer to put forward a systematic historical theory of evolution together with a causal theory involving differential survival in populations of organisms of the same species with different hereditary constitutions (p. 459); F. Sherwood Taylor (p. 475); the reform of the Julian calendar was instituted by Pope Gregory XIII in 1582; the year 1752 is the date of its adoption in England (p. 479). Besides the recent work on medieval science, there are a number of other striking omissions from the bibliographies given at the end of each chapter and at the end of the book. For example, one looks in vain for such basic books as those by Reymond, Brunet and Mieli, and Rey on Greek science, by Koyré on Galileo, by Guyénot on biology in the seventeenth and eighteenth centuries and by Needham on the history of embryology. Neither Sir Edmund Whittaker's *Aether and Electricity* nor Dr E. S. Russell's *Form and Function* appear. Some of these omissions are reflected in Dr Wightman's text, especially his account of the history of embryology and comparative anatomy. A small point: why should Aristotle's theories of spontaneous generation and of circular motion be called 'dogmas' and not theories (pp. 355, 447)?

In conclusion, let me repeat that Dr Wightman's book is one that everyone interested in the history of science should read. I make these detailed criticisms in the hope that they may be of use in the preparation of a revised edition.

A. C. CROMBIE

The Works of George Berkeley Bishop of Cloyne, Edited by A. A. Luce and T. E. Jessop; Vol. IV, edited by A. A. Luce, Thomas Nelson and Sons Ltd., Edinburgh, 1951. Pp. viii + 264. 30s.

THIS volume of the new edition of Berkeley's Works should receive the attention of readers of this *Journal* because it contains Berkeley's significant contributions to the philosophy of mathematics and philosophy of science. These are to be found mainly in *The Analyst* and the *De Motu*.

In *The Analyst* Berkeley criticised the foundations of the differential calculus: he showed with superb clarity that the concept of the infinitesimal was self-contradictory—in brief the infinitesimal was treated both as something and as nothing. Philosophers have tended to discount this criticism, either because they felt that Berkeley could not possibly have been right in his criticism of Newton in Newton's own field, or because they failed to appreciate the force of the criticism and dismissed it as carping. Historians of mathematics, on the other hand, have recognised the validity of his attack. But they did not do so all at once: indeed, fluxions remained a subject of

controversy and without proper foundations for nearly a century until eventually Cauchy settled the problem of limits. Clearly Berkeley's contribution, although of a negative kind, and although he failed to appreciate the soundness of Newton's intuition, was of no mean importance.

The little read *De Motu*, here printed in the original Latin together with a new translation, is of considerable interest, even though it is in some respects a poor composition. It contains one of the earliest modern versions of the descriptive theory of scientific concepts and laws, i.e. that they have a summarising or descriptive function useful for computation and that they have no other rôle or meaning, a view afterwards made fashionable by Mach and many others. The *De Motu* is also notable as containing good criticism of absolute space, motion, and rest (strangely enough, however, absolute time is not mentioned).

The thesis of the *De Motu* may be summarised as follows. Motion is something that can be perceived through the senses ; there is no motion apart from sensible motion ; we find in our observations of moving bodies no active agency that can explain it ; further, there is no gravitation apart from motion ; gravitation is known only in the perception of motion ; to say there is gravitation is to say no more than that things move ; hence gravitation does not explain motion. But, though the concept of gravitation adds nothing to our knowledge, it affords a convenient way of summarising our experience of motion ; mechanics is not therefore useless ; though it gives no clue to the nature of things, it is valuable for calculation. Moreover, motion is not to be thought of as something that happens in a box-like absolute space ; if all objects vanished, space would *ipso facto* vanish also ; hence space is not absolute but relative ; other considerations show that the same holds of motion and rest. Even the phenomena that are commonly supposed to imply absolute motion can be interpreted without it. Lastly, the phenomenon of ' communication of motion ' may be seen not to imply any mysterious abstract motion ; motion and communication of motion are caused and explained by Mind.

To some the more plausible ideas in the *De Motu* may now seem commonplace ; it took a very long time, however, before they became commonplace. The main matter for regret is that Berkeley made no attempt to examine the difficulties or inadequacies of his descriptive view.

The tracts he contributed to the controversy that followed the publication of *The Analyst* make good reading. And in connection with this whole subject the little essay ' On Infinites,' rightly included in the present volume, should also be read. The early mathematical essays, written in Latin, are wholly uninteresting and naive—evidently the editors do not attach great importance to them, for they have printed only the Latin text. Odds and ends of other short writings have an interest of their own but do not bear on the topics discussed in this *Journal*.

It is obvious from this and the preceding volumes that the aim has been perfection of editing and that no trouble has been spared in the attempt to achieve this. The volumes that have so far appeared certainly exhibit very fine scholarship, and we can be sure that those to come will do the same. It is with regret, therefore, that one finds an old error perpetuated ; a mathematical misprint in the first editions of *The Analyst*, repeated in previous collected works under different editors, reappears here (p. 80) without correction or mention.

The editor's introduction to *The Analyst*, in order to depict the value of the tract, unfortunately quotes a misleading eulogy from Gibson. Gibson rightly praised Berkeley's criticism of the infinitesimal, but unhappily continued, ' Berkeley was the first to point out what was again shown later by Lazare Carnot [he should have added Lagrange] that correct answers were reached by a " compensation of errors." ' Berkeley was indeed the first to make this assertion, more precisely that one error in a fluxion was compensated by another in its geometrical application ; but this cannot be true, because the error Berkeley supposed must occur in a fluxion is not in fact present. Gibson's way of putting his remark implies that Berkeley was right, and by quoting it in the introduction the editor may well give the impression that Berkeley's explanation is generally accepted. These blemishes are slight, of course, and would not be worth mentioning were the standard of editing less exacting ; nonetheless they are regrettable, for they could easily have been avoided.

Dr Luce's new translation of the *De Motu* is an asset. The old one, made by Wright, is only moderately accessible. More important, the new translation is a definite improvement : Wright's was lifeless and inexact ; Dr Luce's rendering is exact and reads well.

The editors and publishers are to be congratulated on bringing out such a fine edition ; it is needed.

J. O. Wisdom

Genesis and Geology, Charles Coulston Gillespie. (A study of the relations of scientific thought, natural theology, and social opinion in Great Britain, 1790–1850), Harvard Historical Studies, Volume LVIII, Harvard University Press, Cambridge, Mass. (London : Geoffrey Cumberlege), 1951. Pp. xviii + 316. 30s.

' The English,' wrote a German observer in 1842, ' have a peculiar love of regarding Nature from a theological point of view.' This point of view, strongly advocated by Newton and John Ray, was a frequent object of astonished comment in the eighteenth century by French rationalists, for whom science was the richest source of ammunition to use against a religion in which they no longer believed and a social order of which they

disapproved. Eighteenth-century English and Scottish Protestantism saw no such conflict between science and religion until the geological controversies of the 'nineties. The conflict that began then was a peculiarly British Protestant phenomenon depending on a particular view of the relations between the findings of science and revelations of the workings of Providence. The storm over Darwinism was simply the last, and most violent, of a succession of episodes. In this scholarly, objective and entertaining study, Dr Gillespie has given a clear account of the points at issue and placed an interesting phase in the development of nineteenth-century scientific opinion into historical perspective.

The book consists of an analysis of the opinions of British scientific, and mainly geological, writers from about 1790 to 1850 about the relation between the results of scientific research and the theological notion of God as both the creator and governor of the universe. The central point at issue was always the same, whether it was discussed in terms of the dispute between Neptunists and Vulcanists precipitated by the publication of Hutton's *Theory of the Earth* in 1795, of the Catastrophism of Buckland and Sedgwick as against the Uniformitarianism of Lyell, or of the ' development hypothesis ' of Chambers' *Vestiges of Creation.* The central issue was always the anxiety of pious scientists lest the results of research should show that the argument from design, from the architecture of the universe to a divine Architect, and the argument for a divine Supervisor, were invalid. Nearly all British scientists in this period agreed that science displayed the wisdom and beneficence of Providence and that to do so was one of its most important functions. The trouble was, particularly in the argument for a divine Supervisor who manifested himself by interfering from time to time in the workings of his creation, that the scientists were always cutting the ground from under their own feet. So, as geology progressed, they had to abandon first the Flood, then geological cataclysms in general ; palaeontology pushed the creation of organic species to remote antiquity ; finally, the fanciful Chambers and the scientific Darwin tried to explain the progression of organic forms, including man, by ' natural causes.' As Chambers put it, ' the distinction usually taken between physical and moral is annulled.' All this did not in fact touch the argument from design, and down to Lyell the uniformitarian school consistently asserted that the uniformity, immutability and self-sufficiency of the laws of nature revealed by their work was evidence of God's providential plan. But after Darwin, with a few exceptions, scientific advocates of this argument lost heart. Materialist science finally disposed of a materialist theology.

An interesting sociological point brought out by Dr Gillespie is the astonishing literalness and materialism of the views of pious scientists in early industrial Britain about the manifestations of Providence in contemporary commerce and society. At a public lecture given at an excursion to a lime

quarry during the meeting of the British Association at Birmingham in 1839, Buckland edified the enormous audience of industrialists and working men which had gathered a mile underground to hear him, by asserting that the conveniently placed beds of iron-ore, coal and limestone in the neighbourhood 'expresses the most clear design of Providence to make the inhabitants of the British Isles, by means of this gift, the most powerful and the richest nation on earth.' Popular educators thought that it was only necessary to show the working man the manifestations of Providence in nature, to convince him of the providential organisation of existing society. Of the scientists discussed in these pages, only one, the Presbyterian Highlander, Hugh Miller, author of a geological classic, *The Old Red Sandstone*, saw the dangers of this argument for the ends for which it was put forward, or showed any appreciation of a spiritual religion. Impatiently dismissing what Cardinal Newman was to call 'physical theology,' he took a firm stand on the basic tenets of revealed Christianity, on 'a belief in the immortality and responsibility of man, and in the scheme of salvation by a Mediator and Redeemer. Dissociated from these beliefs, a belief in the existence of a God is of as little *ethical* value as a belief in the existence of the great sea-serpent.' Newman himself said that the results of science had no bearing on the spiritual truths or the historical authority of religion. A similar opinion had in fact been asserted in the twelfth century by the English writer, Adelard of Bath, and was commonly held for some centuries afterwards. It was abandoned, against opposition, by the party in Rome who brought about Galileo's trial, and again by the British Protestants discussed by Dr Gillespie. An objective study of opinion on this question in other periods and countries, placed against the background of historical circumstances, would do much to enlighten present-day discussions of it.

Attention should be drawn to the admirable 'Bibliographical Essay' in 28 pages appended to Dr Gillespie's book. This is a model of arrangement and discussion and contains a most useful set of references to the history of the theory of evolution. A small point: on p. 103 in a quotation from Buckland 'honour' is misspelt 'honor.'

A. C. Crombie

Studium Generale. Zeitschrift für die Einheit der Wissenschaften im Zusammenhang ihrer Begriffsbildungen und Forschungsmethoden, Hefte 1–7, 1951. Edited by M. Thiel, Springer-Verlag, Heidelberg. Foreign Agents: Lange, Maxwell and Springer, London. Each part about pp. 64. DM. 4.80–6.60.

This important journal, appearing approximately monthly, was founded in 1947, but is still little known in this country. As the sub-title indicates, its field is the unity of all branches of learning (or systematic

knowledge, *not* science), their concepts and methods of research. The editorial council includes representatives of twenty-four subjects, from theology and philosophy, through the natural and human sciences, to languages, history, economics, and technology. So far the authors appear to be all German-speaking, and the writing may be assumed to be representative of the best German-language scholarship.

Some of the parts are devoted to the treatment of a single theme from several points of view, though this principle is not followed strictly. Here are examples of topics covered which lie within the scope of *The British Journal for the Philosophy of Science :* Experimental method (Vol. 1, Part 1) ; Scientific Models (1, 3) ; Causality (1, 6) ; Rhythm (2, 2, 3) ; Symmetry (2, 4, 5) ; Environment and other biological problems (3, 2, 3) ; Depth Psychology (3, 7) ; Probability (4, 2) ; Typology in biology and psychology (4, 7, August 1951). A few articles appear to have wandered into neighbouring issues, and those interested should obtain the printed list of contents for the years 1947-50.

One part may be considered in detail. The issue for July 1949 is one of the most interesting and up-to-date surveys, suggestive rather than systematic or comprehensive, of the role of symmetry in the exact sciences and art. W. v. Engelhardt opens with a general paper on symmetry in geometry, physics, and ornament ; K. L. Wolf discusses symmetry and polarity, introducing a valuable extension of the standard conception of symmetry ; P. Niggli considers symmetry in the natural sciences and raises epistemological issues ; W. Ludwig and W. Troll, in two useful papers, present contrasted views of symmetry in biology ; W. Troll treats beauty in relation to teleology, Goethe, and d'Arcy Thompson ; and finally D. Frey examines symmetry in the plastic arts.

The issue for February 1951 on Probability is ' probably ' of equal value, by which it is meant, following Laplace, that there is no known ground for this not to be the case, since it opens with a paper by B. L. van der Waerden on ' The Concept of Probability ' and contains six other papers on different aspects of the subject.[1]

But these examples do not reveal that rather more than half the journal is concerned with historical, religious, and cultural topics. The implicit aim of many of the contributors (perhaps not fully conscious) appears to be the promotion of a unification of knowledge in conformity with the subjective-idealist-' humanist ' trend which has dominated much German thought in the past. Certainly so vast a field can only be seen as a unity if some one method of approach is chosen, and the need for a rediscovery or deeper vision of the unity of knowledge is manifest. *Studium Generale* will therefore receive a lively welcome from all who are aware of this need.

[1] An expert states that it does not display knowledge of important advances made outside Germany during the last twenty years.

It may be useful to indicate more precisely what appears to be the real intention of the journal. The declared purpose is the unity of all learning, but, unless a sampling has misled, this is conceived on a somewhat restricted basis. If the primary aim were *the development of scientific truth* (in the sense of ordered and objectively tested knowledge) a more cautious step-by-step approach would be appropriate, guided by a searching critique of fundamentals. (For example, there appears to be inadequate awareness of the results of mathematical logic and semantic analysis in recent decades, particularly in relation to the use of language in scientific exposition.) On the other hand, if the aim were *a unified foundation for a human society open to all*,[1] a universal rather than a nationally coloured approach would be necessary. For this journal, in its special philosophical assumptions, its rather undiscriminating apparent comprehensiveness, and its relative neglect of recent foreign work, is evidently the product of an unduly isolated Germany. The most reliable path of advance towards a universally acceptable synthesis of culture no longer lies through separate national efforts, but involves a more objective, international, and genuinely comprehensive approach. Whereas the aim here seems to be *the unity of a German system of thought.*

It has taken four years for this new journal to be reviewed in this country and there seems also to be a serious delay in foreign work being studied by German scholars. British and German universities, by improving the exchange of literature, should break down the petty barriers which still prevent Europe playing its full part in intellectual endeavour.

L. L. Whyte

Doubt and Certainty in Science, J. Z. Young, The Clarendon Press, Oxford, 1951. Pp. viii + 168. 7s. 6d.

Professor Young's broadcast Reith lectures, here reprinted along with a series of commentaries about equal in length to the spoken text, form a highly original work. The title will prove somewhat misleading. The book hardly deals with the problem of when (if ever) we are certain of a scientific conclusion. It adumbrates a new type of metaphysics. As, however, the book is aimed at a very wide public, the full implications of this metaphysics are not developed.

The standpoint adopted is shown by the following quotation (p. 8): 'The method that I am going to suggest as a working basis is to organise *all* our talk about human powers and capacities around knowledge of what the brain does.' The author carries out this programme to a considerable

[1] This assumption would be in accordance with *University Reform in Germany.* Report by a German Commission. Foreign Office. 1949. H.M.S.O. See Section 5 on *Studium Generale.*

extent. He has examined a large number of brains (some alive, but mostly dead), and tries to give an account of the world taking the brain for granted. Other philosophers have based their systems on 'atoms and the void,' or some other account of matter in general, on 'les données immédiates de la conscience,' or some other aspect of human consciousness, and arrived at various forms of materialism and mentalism. Professor Young's philosophy, which we may call cerebralism, appears to be a rather aberrant type of materialism, for he is concerned with the brain, not so much as the seat of consciousness, but as the organ through whose use human beings behave as such, and in particular communicate with one another. No Christian, at any rate, after reading the first verse of St John's Gospel, can object to the emphasis laid on communication.

The account of the structure and functions of the brain is extremely clear, though inevitably sketchy. To me, as a former pupil of Sherrington's, the most serious omission seemed to be any reference to Hughlings Jackson's conception of the relations between levels. Jackson held that an important function of the higher (literally so when we are standing up) parts of the brain was the inhibition of the activities of the lower parts, so that the destruction or inactivity of a higher part led to release of function, for example uncontrollable fits of rage, or an uncontrollable standing posture (decerebrate rigidity). And personally I find the treatment of pain on pages 115 to 119 unconvincing. Young regards pain as due to sets of nervous impulses which lack a definite pattern, so that we cannot fit it in 'to our set of rules.' I regard pain as a particular kind of sensation about which I can lay down rules; for example that a toothache is less like an itch than is a burn; and which becomes distressing when it rises above a rather low level.

For Professor Young doubt is an active cerebral process of searching for regularities, essentially similar in a man blind from birth and first seeing as an adult, and in a physicist trying to make sense of data on mesons; and certainty is the somewhat relative certainty which we reach when we can say, 'this is a table,' or 'that was a π-meson,' until a new source of doubt appears. Unfortunately he does not tell us in any detail how we reach certainty, that is to say how our brains construct a series of rules, models, or schemata, with which our current experience concords. This is because he does not know, nor does anyone else. The results of local injuries make it quite clear that these memories, models, universals, or engrams, are not located in any one place in the brain. Until a little more is known on this matter cerebralism will find it hard to meet such criticisms as those in Bergson's *Matière et Mémoire* very completely. In fact, cerebralism will be a programme, as of course it is for Professor Young, rather than a philosophy.

From this rather insecure base the author sallies out to attack both mentalism and materialism. 'Subjective language is defeatist' and

'Physics is no longer materialist,' because 'we no longer speak of a world of matter, nor of particles, properties, or forces.' Now before turning down materialism it might have been well to consider the account of it given by the majority of people who call themselves materialists, namely those who adhere to the teachings of Marx, Engels and Lenin. Here are two quotations from Lenin's *Materialism and Empirio-criticism*: 'For the sole "property" of matter, with the recognition of which materialism is vitally concerned, is the property of being objective reality, of existing outside of our cognition.' 'It is, of course, totally absurd that materialism should maintain the "lesser reality" of consciousness, or should necessarily adhere to a "mechanistic world picture" of matter in motion, and not an electromagnetic, or even some immeasurably more complicated one.' I fear that on Lenin's criteria physics are still somewhat materialistic, and Professor Young, who does, I think, 'maintain the lesser reality of consciousness,' is a somewhat narrower materialist than was Lenin.

In fact, of course, 'the form we give to this world is a construct of our brains' (p. 107). Our account of the brains is equally such a construct, so Professor Young, like any philosopher who concentrates on one aspect of the universe, whether it be consciousness or elementary particles, inevitably saws off the branch on which he is sitting if he pushes his programme to its logical conclusion. Nevertheless, the book shows that the language of cerebralism is useful. It allows the author to answer a number of questions concerning human conduct, including religious practice and belief, in a somewhat novel way. It is, therefore, a contribution to human thought, and it is likely to be an important one, because our knowledge of cerebral processes is growing fairly quickly. In particular Young stresses the possibility that a knowledge of the process of learning may enable many people to learn a great deal more. But his outlook has its dangers. 'We lack a general scheme for mankind as a whole. Evolutionary theory provides that general plan,' he writes. It can equally be argued, with T. H. Huxley, that man's hope lies in the 'ethical process,' which runs counter to the cosmic or evolutionary process. There is a very real peril in any attempt to base ethics on the evolutionary process as at present understood in terms of natural selection. The alternative of trying to describe the evolutionary process in ethical terms could be equally harmful to ethics and to biology.

It will be seen that the book raises a great many questions, and represents a trend in human thought which is likely to continue, and to be fruitful. For both these reasons it is very well worth reading.

J. B. S. Haldane

BOOKS RECEIVED FOR REVIEW

Inclusion of books in this list does not preclude their being reviewed in later issues

J. D. Bernal, *Marx and Science*, Marxism Today, Series No. 9 (Foreword by Professor Benjamin Farrington), Lawrence and Wishart, Ltd., London, 1952, pp. 56, 2s. 6d.

H. Bondi, *Cosmology*, Cambridge University Press, 1952, pp. ix + 179, 22s. 6d.

W. Russell Brain, *Mind, Perception and Science*, Blackwell Scientific Publications Ltd., Oxford, 1951, pp. vi + 90, 6s.

Rudolf Carnap, *Logical Foundations of Probability*, Routledge & Kegan Paul, Ltd., London, 1952, pp. xvii + 607, £2 2s.

Haskell B. Curry, *Leçons de Logique Algébrique*, Gauthier-Villars, Paris, 1952, pp. 163, Fr. 1,600.

F. S. A. Doran, *Mind—A Social Phenomenon*, C. A. Watts & Co., Ltd., London, 1952, pp. 182, 10s. 6d.

Philip G. Fothergill, *Historical Aspects of Organic Evolution*, Hollis and Carter, Ltd., London, 1952, pp. xvii + 427, 35s.

R. W. Gibson, *Francis Bacon—A Bibliography*, Scrivener Press, Oxford, 1950, pp. xvii + 369, £7 7s.

John MacPartland, *The March toward Matter*, Philosophical Library, New York, 1952, pp. 80, $2.75.

Leland Mathis, *Notes on the Theory of Progress*, Pine Avenue Publisher, Riverside, Illinois, U.S.A., 1951, pp. ii + 63, $1.00.

E. A. Milne, *Modern Cosmology and the Christian Idea of God* (Edward Cadbury Lectures in the University of Birmingham for 1950), Clarendon Press, Oxford, 1952, pp. 160, 21s.

Richard von Mises, *Positivism*, Harvard University Press (London : Geoffrey Cumberlege), 1951, pp. xi + 404, 40s.

Andrzej Mostowski, *Sentences Undecidable in Formalized Arithmetic ; An Exposition of the Theory of Kurt Gödel* (Studies in Logic and the Foundations of Mathematics), North-Holland Publishing Co., Amsterdam, 1952, pp. 117.

Radhakamal Mukerjee, *The Dynamics of Morals*, Macmillan & Co., Ltd., London, 1952, pp. xxviii + 530, 25s.

Martha Ornstein, *The Rôle of Scientific Societies in the Seventeenth Century*, University of Chicago Press (Agents in Gt. Britain : Cambridge University Press), 3rd ed., 1938, pp. xviii + 308, 22s. 6d.

I. P. Pavilov, *Scientific Session on the Physiological Teachings of Academician I.P. Pavilov*, 1950, Foreign Languages Publishing House, Moscow, 1951 (distributed in Gt. Britain by Collet's Foreign Department, London, 1952), pp. 174, 2s. 6d.

K. R. Popper, *The Open Society and Its Enemies* (Vol. 1 : The Spell of Plato, Vol. II : The High Tide of Prophecy : Hegel and Marx) Routledge & Kegan Paul Ltd., London, 1915. Reprinted 1947, 1949, 2nd ed. (Revised) 1952, pp. Vol. I, 318, Vol. II, 375, £2 2s.

Giovanni Ricci, *La Relativita tramonta—Gravitazione : mistero svelato Spazismo*, Tipografia Morini-Reggio Emilia—via R. Franchelli, 1951, pp. 63, L. 350.

BOOKS RECEIVED FOR REVIEW

Lynn Thorndike and Francis S. Benjamin, Jr. (editors), *The Herbal of Rufinus*, Chicago University Press (agents in Gt. Britain : Cambridge University Press), 1946, pp. xliv + 476, 37s. 6d.

Lynn Thorndike, *Latin Treatises on Comets Between* 1238 *and* 1368 *A.D.*, Chicago University Press (agents in Gt. Britain : Cambridge University Press), 1950, pp. x + 275, 37s. 6d.

J. O. Wisdom, *Foundations of Inference in Natural Science*, Methuen & Co. Ltd., London, 1952, pp. x + 242, 22s. 6d.

C. F. von Weizsäcker, *The World View of Physics*, translated by Marjorie Grene, Routledge & Kegan Paul, Ltd., London, 1952, pp. 219, 12s. 6d.

Maurice de Wulf, *History of Medieval Philosophy*, Vol. I, translated by Ernest C. Messenger, Thomas Nelson & Sons, Ltd., Edinburgh, 1952, pp. xviii + 317, 21s.

Rudolf Zocher, *Leibniz' Erkenntnislehre*, Walter de Gruyten & Co., Berlin, 1952, pp. 33, DM. 4.80.

NOTES

Sir Charles Sherrington, O.M., F.R.S.—With the death of Sir Charles Sherrington on 4th March, 1952, the *Journal* lost a distinguished member of the Editorial Board. An obituary notice will be published in a later issue.

Institute for the Unity of Science.—The Institute for the Unity of Science is offering a prize of $500 for the best essay on the theme 'Mathematical Logic as a Tool of Analysis: Its Uses and Achievements in the Sciences and Philosophy.' Two additional prizes of $200 each will be given for the next best two essays. It is an International Contest and is open to everyone. Essays must not exceed 25,000 words. They may be written in English, French or German, and must be submitted before 1st January 1953. Further information can be obtained from the Institute for the Unity of Science, American Academy of Arts and Sciences, 28 Newbury Street, Boston 16, Massachusetts.

Recent Publications on the Philosophy of Science. It is proposed in future issues to take steps to provide readers of the *Journal* with information about recent publications in the field of the philosophy of science and the history of scientific ideas. The present arrangement, by which advertisements showing the contents of each issue are exchanged with other journals dealing with cognate subjects, is being extended. In addition, it is proposed to include in each issue of the *Journal* a section entitled *Some Recent Publications on the Philosophy of Science.* This section will subsume the present *List of Books Received for Review.* Authors of articles are invited to send offprints to the Editor, and readers are invited to send details of any books and articles they may come across which they consider suitable for inclusion in this section. The details mentioned should be, in the case of books: the full name of the author, title of the book, and name of the publisher, as given on the title-page; place and date of publication; number of pages, with preliminary pages shown separately in Roman numerals; and price. If the book is part of a series the title of this should be given in full. In the case of articles the details mentioned should be the full name of the author and title of the article, as published; the full name of the journal or collection in which it appeared, and the place of publication; and the year, volume, and number of the first and last page of the article. It will help the Editor if correspondents will observe the form of giving references used in the *Journal*. The fact that the name of a book or article has been missed in one issue will not preclude its being referred to in a later issue.